THE

AMERICAN JOURNAL OF SCIENCE.

[THIRD SERIES.]

ART. XXII.—*On Lower Silurian fossils from a limestone of the original Taconic of Emmons;* by JAMES D. DANA.

[Read at the meeting of the American Association at Ann Arbor in August, 1885.]*

THE Lower Silurian fossils hitherto reported from rocks of the Taconic system have come either from localities to the north of the original Taconic area, in the State of Vermont, or from others to the southwest of it, in Dutchess County, New York. The discovery of fossils which I have now to announce was made the present season in a limestone of the original Taconic series of Professor Emmons, his "Sparry limestone," within a mile and a half of the Massachusetts boundary.

That the facts may be fully understood as to what is the *original* and, therefore, *true* Taconic, a brief review of the subject is here introduced; for mistakes are still continually made by writers on the subject.

Professor Emmons, in his first account of the "Taconic System," which makes thirty pages of his Report (in quarto) on the Geology of New York, published in 1842,—mentioned as the constituent rocks, commencing on the west: (1) "Taconic slate," in eastern New York, including the Hoosic slates;† (2)

* Some changes have since been made in order to include the facts from additional observations on the fossils and rocks of the region.

† Of this division of "Taconic slate," his No. 5, in the tabular statement on page 144, Prof. Emmons says, on page 150, "No organic remains have been found in the rocks; it is even destitute of those obscure markings which are called *fucoids*."

the "Sparry limestone" interstratified with the slates west of the Taconic Range and for the most part lying against the west side of the range; (3) The "talcose" or "magnesian" schist (as he called it) constituting the Taconic Range, and Greylock or Saddle Mountain, the high ridge between Williamstown and Adams in the northwestern angle of Massachusetts; (4) the Stockbridge limestone, east of the range of Taconic schist; (5) quartzyte.

He described the prevailing dip of the strata as eastward, as later investigators have done. He made the sparry or western limestone older than the Taconic schists, the limestone dipping under the schists. The belt of sparry or western limestone (sparry is a misleading term and was afterward rejected by him) he described as extending through the New York towns of Hoosic, Petersburg, Berlin, New Lebanon, Canaan and Hillsdale, along the west side of the Taconic Range.

The above-mentioned series of rocks constituted the Taconic System as propounded and described by Prof. Emmons in 1842. He says, on page 136 of this Report:

"The Taconic System, as its name is intended to indicate, lies along both sides of the Taconic Range of mountains, whose direction is nearly north and south, or, for a great distance parallel with the boundary line between the State of New York and those of Connecticut, Massachusetts and Vermont." He remarks that the rocks extend farther north through Vermont into Canada, but gives no details about Vermont or Canada localities, and adds, "It is, however, in Massachusetts, in the County of Berkshire, that we find the most satisfactory exhibition of these rocks."

Prof. Emmons's declaration, in this first detailed account of the system, as to what is the typical Taconic series, must be accepted; and the geological age of these beds—the rocks of the original Taconic System—whatever it may prove to be, must therefore be taken as the age of the Taconic System.

The stratigraphical observations of Prof. Emmons are right. But he took a step of questioned correctness when he concluded in 1842 that the system was intermediate in age between the New York Potsdam sandstone and what he calls the "Primary" system lying to the eastward; or, as stated, in another place (on page 163) that the rock "appears to be equivalent to the Lower Cambrian of Sedgwick." For the criterion of age which he announced, namely: "As a general rule, certain minerals are found in particular rocks" was of unascertained value; and the facts referred to as a basis of his conclusion: that the Taconic quartzyte is interstratified with limestone; that the limestones are peculiar in grain; and others of related character, are doubtful evidence for separating them

from corresponding New York rocks. He says (p. 139), "we find no fossils in the rocks of which I am speaking, and hence can derive no aid from that source upon which any reliance can be placed." He states no case of unconformability with the New York rocks. In a later publication he refers to a case of *dislocation*, as he terms it, in the Hoosic region, but not to any of true unconformability. Dislocation, that is, faulting, it is well known, proves nothing as to distinction of system; and the particular facts at Hoosic referred to are proved to be of merely local interest, by the general range of facts in the Taconic region.

Through the use of his lithological canon,—*his*, for he was the first to announce it—Professor Emmons had identified, by 1844,* areas of Taconic rocks in Rensselaer County, New York, Northern Vermont, Canada, Maine, Rhode Island and Michigan; and later, in New Jersey, Virginia, North Carolina and Georgia; and others have used the canon since with equal success, even sometimes at *very* long range.

In saying that such a use of lithological distinctions was unwarranted, I mean to assert that geological investigation with reference to the canon had not advanced so far as to make its application safe, and render it certain that fossils might not yet be discovered to upset the conclusion. I mean to assert, further, that the conclusion *does not* now stand against actual discoveries of Silurian fossils later than Potsdam that have recently been made.

The history of Taconic geology suggests caution with regard to the use of lithological distinctions as evidence of geological age. It shows how easy it is, under such prompting, to put assumptions for facts, to say "I know" when one ought to say "I doubt," and to build up systems that are likely to fall to pieces.

Professor Emmons, after he had found slates in Washington County, New York, *looking like Taconic*, and which he called Taconic, made the discovery in them of trilobites, large trilobites, which Barrande later pronounced to be *Primordial* species. This was evidence to him that these beds were of later age than the typical Taconic, and so he made for them the subdivision of *Newer* Taconic. The discovery was a grand one, and may well give us a high opinion of Prof. Emmons as a geological observer. But the conclusion that the rocks were rocks of his Taconic system had not been proved.

Prof. Emmons, in his American Geology, where he makes his latest exposition of the Taconic subject (covering with it

* Chapter on the Taconic System of his N. Y. Agricultural Report (1846), issued in advance of the volume under date of 1844.

† "American Geology," by Prof. E. Emmons. 1855, Part II, p. 40.

122 pages of the volume), includes in the *Lower* Taconic, "as developed in Williamstown and Adams;" the quartzyte; the schists of the Greylock ridge and others west; the Stockbridge limestone; and, with these, the western or "sparry" limestone and the associated slates; that is, all the true original Taconic. To the Upper Taconic (p. 49) he refers the black slates and calcareous beds at Easton, Washington Co., N. Y., which, four or five miles north of Bald Mountain, afforded him the Primordial trilobites; similar slates and limestones at St. Albans, with some sandstones. He also includes the slates and limestone of Poughkeepsie and of the region east of there to Sharon, Connecticut—beds which have recently been proved to contain abundantly, in some localities, Hudson River, Trenton and Calciferous fossils (made known from the Poughkeepsie slates first by Mr. Nelson Dale, and from the limestones, by Prof. W. B. Dwight and the author), and which recent discoveries of Prof. W. B. Dwight prove to be also in part of Potsdam age, as a paper of his, prepared for the present meeting of the Association, announces.*

Now the larger part of all that has been written on the Taconic question is about the outside rocks—the "Newer Taconic." Volumes have been devoted to them which have settled nothing. Emmons did not make those Primordial slates Taconic by calling them so, any more than Dr. C. T. Jackson made the Red sandstone of the Lake Superior region Triassic by calling it so, because the red sandstone of the Connecticut Valley had been determined to be Triassic. Should it be substantiated that the original Taconic of Emmons is in part of Calciferous, Trenton and Hudson River age (that is, Lower Silurian), what has been gained from the effort to prove the Primordial slates of Washington County, N. Y., to be not only Taconic in system, but Taconic *newer* than the original Taconic?

We have now unimpeachable evidence—evidence from fossils—that those Primordial Trilobite beds are *not newer* than the typical or original Taconic; that the limestones of the true, original, typical Taconic, on the contrary, are the newer. The Lower Silurian fossils here referred to were found, as has been stated, in the "sparry" or western limestone of the Taconic System—the oldest limestone stratum of the system according to Emmons in 1842. They are from Canaan, New York, the town next south of New Lebanon. The Boston and Albany railroad, as it crosses the State-line in leaving Massachusetts for Albany enters the town of Canaan and its limestone area; and the fossils have been collected from a cut along the railroad, and from ledges of limestone half a mile south of the road and a mile north of it.

* Published at p. 125 of this volume.

The limestone of this belt is mostly a gray, fine-grained crystalline rock, varying to whitish on one side, and to blackish, faintly crystalline, on the other. Short lines and blotches, suggestive of fossils, are common in its grayer portions, but distinct forms I had failed to find. Hope was renewed during the past winter by receiving, from Mr. D. Clark, of Tyringham, a piece of obviously fossiliferous limestone labeled "West Stockbridge." It led to an unsuccessful search in that town in which I was aided by Prof. Dwight of Poughkeepsie. On his return homeward, Prof. Dwight continued his search in Canaan, N. Y., the next town west, and there he had success. This careful investigator found fossils in the limestone at two localities, one at the railroad tunnel, southeast of Canaan Center, and the other, half a mile to the south, in a feebly crystalline blackish variety of the limestone. I was shortly afterward in Canaan for further search and study of the rocks, and also at other times later; and I discovered evidences of fossils also in the limestone in place two miles east of Canaan Center, on the farm of E. S. Hall, but none as distinct as those of the localities first visited by Prof. Dwight.

Prof. Dwight by slicing and polishing has brought to light several determinable species, notwithstanding the metamorphism of the rock. The paper following this contains the results of a careful study of them by himself and Mr. S. W. Ford; and Plate VII, illustrating it, shows at a glance, that this Taconic limestone is not pre-Cambrian or Cambrian. The species include remains of Murchisonias, Pleurotomarias, Crinoids, Fenestellæ, a Trilobite, and probably of Brachiopods; and multitudes of fragments not determinable. From the fossils, these authors are led to refer the limestone, that is, the part of it affording the fossils, probably to the Trenton period.

Two of the localities affording fossils occur in the main belt of the limestone, where it is half a mile to a mile wide, and the third (the locality at the railroad tunnel) in a limestone area on the west side of an isolated belt of schist or slate, which connects with the main belt two miles north, near Queechy; and they are all within two miles of the Massachusetts boundary and a mile or less from the schist of the Taconic range. The distribution of the areas and the positions of the localities will be given on the map of Middle and Northern Berkshire which I shall soon publish in this Journal.

At the railroad tunnel the limestone dips eastward at a small angle (15° to 20°) beneath the slates; and the locality that has afforded the fossils is hardly 200 yards from the overlying slates on the east and south. Both of the localities found by Prof. Dwight are very near this isolated ridge of schist, that of the tunnel limestone on the west of it, and the other ledge

near the east side; and they render probable, if not certain, the overlying position of the schist in this ridge.

But the fossils above mentioned are not those of the specimen of limestone received from Mr. D. Clark. That specimen was given him by Mr. J. W. Smith, of Canaan-4-Corners, and came from the farm of Mr. E. S. Hall above referred to. Unfortunately, all that is now exposed to view is in three loose angular masses, two feet in cubic size and smaller, and not a ledge.

A fourth larger mass was till recently in sight; but it has had its upper part removed to a considerable depth below the suface in clearing the farm of its rocks, and I was informed by Mr. Smith, that in the removal two wagon loads of blocks were carried off, though a large portion was left under the soil. As the farm was under cultivation through the season, I could not have an opening made. An attempt to find the underground mass made since I left the region has proved unsuccessful; so that it will have to remain undiscovered until another season opens. As to the origin of the four masses, all I can say now is that, if transported, they are not probably from any point outside of the valley; for they are angular masses, little worn, and lie all within fifty feet of one another. As the rock is a porous one, it would wear more easily than other portions of the limestone and hence be likely to be covered with soil.

Although the evidence from these specimens is not yet available because of the doubt as to locality, it is of interest to note that they are almost identical in character with the fossiliferous Wappinger valley limestone (part of Emmons's Western Taconic limestone) at Pleasant Valley, a few miles northeast of Poughkeepsie. Like that, it consists largely of nodules of so-called *Chætetes compactus* (now shown by Mr. Dwight to be a *Solenopora*), which give it the aspect of a conglomerate. Along with this fossil, the eye easily detects disks of crinoids, true Chætetes, and in one case a portion of a large Receptaculites, similar to that found with the same limestone at Rochdale near Poughkeepsie. Prof. Dwight has made slices of the rock and says that it contains all of the kinds of fossils occurring in the Pleasant Valley limestone, and others observed by him only at other localities in that region. There is no doubt of its Trenton age.

In former papers I have presented evidence, gathered by myself and earlier observers, proving that the Taconic range and the adjoining limestones on the east and west, constitute, in the main, a great synclinal or compound synclinal. It hence follows that fossils of the western limestone of Canaan indicate the Lower Silurian age of the eastern or Stockbridge limestone.

But, more than this, the eastern and western limestones have a surface connection. From New Lebanon the western continues northward, and three miles to the north (in Stephentown) it stretches on, in a north by east direction, into Massachusetts, passing in a broad belt through the towns of Hancock and Williamstown, as my Berkshire map, above alluded to, will show, confirming Professor Hitchcock's map of 1841. Thence it extends northward *uninterruptedly* into Vermont (as the Vermont geological survey, and my own observations prove), and becomes the great central marble belt of that State, passing through Bennington, Dorset, Rutland, and to and beyond Middlebury. Thus the same Taconic limestone belt that contains Trenton and Chazy fossils in Rutland, Vermont and towns farther north, as shown by the Vermont Geological Survey, is now proved to have Lower Silurian fossils west of the Taconic range at Canaan in eastern New York.

The reading of this paper at the meeting of the American Association was followed by remarks by Prof. James Hall and also by Prof. N. H. Winchell. Prof. Hall observed rightly that the conclusion as regards the Lower Silurian age of the Taconic rocks was not new. This fact I have urged in all my papers on the Taconic rocks except the present one; and I made it the special point of the paper preceding this one, presented in 1882 to the Geological Society of London, showing that the conclusion I held had been sustained by every geologist who had investigated the region and published on it since the Taconic system was first announced—W. B. and H. D. Rogers, W. W. Mather, E. and C. H. Hitchcock, and A. Wing. In my Manual of Geology, the bibliography of the Taconic system is given under the two heads of authors sustaining its Lower Silurian age, and those not; and among the former is the name of Prof. Hall.

Unfortunately for the science, Prof. Hall, while one of the foremost to contest the conclusions of Prof. Emmons before the Association of Geologists and Naturalists in 1840 to 1845, published nothing on the subject; neither were his remarks before the Association published in its brief proceedings. Moreover, no paper giving his later opinion has since appeared. In 1884 (September 13), 40 years after those discussions, I received from him (as a sequel to a letter of enquiry as to any publications of his on the subject) two plates of stratigraphical sections across the Taconic region, prepared by him 40 to 45 years since, showing plainly careful labor in the field with reference to settling the debated question. This most welcome addition to the facts afforded proof conclusive of the synclinal character of portions of the Taconic range, as I have stated in a notice of them in volume xxviii (1884) of this Journal (p. 311). I had also asked him in my letter his opinion as to the age of the Hoosic Slates (40 miles north of Canaan), from which Prof. Emmons and others, in his publications later than 1842, had reported Graptolites, this point bearing

directly on the age of the accompanying limestones. In his reply, as I report in the notice (on p. 312), he said that he was at "present uncertain as to the precise period of the Hoosic Slates." Prof. C. H. Hitchcock, in 1884, had referred to the fossils as proving the Lower Silurian age of the limestone, and in a letter of the 30th of January last, says: "I do not recall any fossil there except the graptolites."

At the time of the earlier discussions in 1840 to 1844, in which Professors Hall, Rogers, Mather and others, took part, the only Taconic rocks under consideration were, as Prof. Hall remarked in a letter to me of last September, those of the true Taconic range, as defined in the above paper. I had no part in the discussion; in fact, I knew nothing about the Taconic except what I learned from the discussions after 1842, owing to my absence from the country for the four years previous; my first published opinion on the question is in my Manual of Geology (1863) in which Logan's conclusion was adopted.

The report of Prof. Hall's remarks at Ann Arbor implied that he spoke of having known of fossils from Canaan 40 years since.

But, in a letter received soon after, he informed me that he was misunderstood and that he referred to fossils from the limestone of Hoosic. The graptolites of the Hoosic slates are the only fossils of that town mentioned by Prof. Emmons or others in the various controversial writings on the subject; and as no notice of any fossils in the Hoosic limestone had ever been published, I enquired in another letter as to the species, in order to make the argument from the facts more satisfactory, but have not yet received the desired information. It is to be hoped that they will soon be announced, that the evidence as to the age of the Taconic schist may be of the fullest possible character.

ART. XXIII.—*Preliminary Report of* S. W. FORD, *and* W. B. DWIGHT, *upon fossils obtained in* 1885 *from Metamorphic Limestones of the Taconic Series of Emmons at Canaan, N. Y.* —(With Plate VII.)

A. *Explanatory statements with reference to the paleontological investigations at Canaan, N. Y.*, by W. B. DWIGHT.

IT seems proper to preface the Report on the fossils found in 1885 at Canaan, N. Y., by a few explanatory statements which cannot well be included in that paper.

My brief paleontological observations in the above mentioned district have been made by the valued invitation of Prof. J. D. Dana, in connection with his comprehensive study of that portion of the Taconic range. At the locality where fossils were first discovered, about 2400 feet southerly from the railroad tunnel, only the smaller organisms have as yet been detected. These are very difficult to observe, and therefore to collect,

especially when obscured by the dust of a fresh fracture. The difficulty is much increased by the fact that the ledge is in the shade of thick woods.

In this place I have noticed only one general phase of the rock, that of a massive heavy-bedded limestone, which in many parts has been broken into small fragments and re-cemented by a close network of veins. Although this is a discouraging point for collection, on account of the rough, moss-covered character of the crags, in addition to other difficulties already mentioned, yet it has proved as rich in the smaller organisms as the tunnel locality, if not richer.

At the western end of the railroad tunnel, the features are somewhat different. There is more open ground, and the collecting is far easier. In addition to many of the smaller organisms, there are quite a number of large conspicuous fossils. There is also more variety in the texture of the rock than in the first mentioned locality. The narrow fossiliferous belt which crosses the western roof of the tunnel, presents two varieties; for the width of a few feet along the extreme western edge, the rocks are generally light-colored, quite calcareous, schistose, and inclined to produce in weathering, quite smooth, whitish surfaces. These portions contain the larger and more prominent fossils. The strata lying more above and to the east, are darker, and though highly calcareous, yet much interspersed with delicate shaly or arenaceous layers. In these portions, more particularly where the texture is somewhat shaly, there are films crowded with crinoidal fragments, and delicate gasteropods. The lighter-colored strata do not, however, appear to be quite destitute of such fossils as abound in the others, and I find no good reason for supposing that there is any distinction here of stratigraphical value.

Without extensive application of the processes of polishing and making transparent sections of the specimens here collected, it would have been impossible to have arrived at any satisfactory knowledge of the fauna present.

My paleontological observations at these localities have been brief and entirely preliminary. Having visited the place only on three separate days, I have spent, in all, not more than eight or nine hours at these fossiliferous localities. That so large and interesting a collection should have been made in so brief a time shows how numerous must be the remains still preserved in this crystallized rock.

In view of the difficult and fragmentary character of the material, even after I had most thoroughly developed it, it has been a great gratification to me that Mr. S. W. Ford has consented to participate in the final examinations, and in making a joint report.

B. *Joint Report on the Fossils.*

The fossils which form the subject of this joint examination and report were collected by W. B. Dwight on the days of June 12th, June 23d and October 31st, 1885, at the following localities of metamorphic limestone: (1) The western roof of tunnel No. 192 of the Boston and Albany R. R. at Canaan, N. Y.; and (2) a ledge situated about 2400 feet southerly from the above tunnel. Subsequently, a few slabs of fossiliferous rock from the tunnel were sent from Canaan through Professor Dana, which have furnished us considerable additional interesting material. From similar limestone obtained during the same season by Professor Dana, on the farm of E. S. Hall, about two miles northeast of the tunnel, we have also obtained some evidence of organic structure. Although a thorough paleontological study of the region has not yet been made, and our report must therefore be regarded as only preliminary, enough has nevertheless been seen to convince us not only of the Lower Silurian age of the strata, but to enable us to fix, with a considerable degree of probability, their exact horizon.

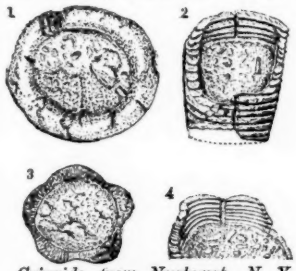
Evidences of the extensive metamorphism of the strata have been constantly met with. The rock, though often abounding in fossils, yet presents them, for the most part, under such conditions as to render their determination a matter of extreme difficulty. The organisms are, to a great extent, in an exceedingly comminuted state, and frequently present the appearance of having been partially dissolved. Many hand specimens of the limestone, and especially transparent sections of it made with much care, appear to strongly indicate the former semi-fluidal condition of portions of the strata. In other cases, however, where the rock has been less altered, the fossils are quite perfect and sharply defined. The greater portion of the specimens which we have studied, have been brought to light by means of polished surfaces and transparent rock slices, although a number of them are large and conspicuous fossils. In view of the important bearing of the facts with which we have had to deal, we have exercised great care in our examinations and have spared no pains in our endeavors to arrive at safe conclusions.

We have found, in the material under consideration, the following organisms.

ECHINODERMATA.—The majority of the slices and polished surfaces prepared by Professor Dwight from the layers in which the smaller organisms occur, are replete with fragments of small crinoidal columns of forms similar to those so frequently met with in well-known localities of the Trenton limestone. Figures 1 and 2 of Plate VII, of which the first is a transparent

section and the second a polished rock surface, illustrate this fact, and are not exceptional in character. Other slides show these features less perfectly, while in still others their definition is even sharper, as in figs. 3 *a–b*. This latter form we think may represent the column of some known species of *Heterocrinus*. Associated with it there occur the forms shown in figs. 3 *c–f*, which we look upon as joints of encrinal stalks; also the forms represented by figs. 3 *g–j*, which we are disposed to regard as more or less oblique longitudinal sections of the same or similar columns. All of the forms illustrated by figs. *a–j*, have been brought out by means of transparent rock slices, and so strongly resemble, in many respects, certain forms shown in similar slices of limestone of undoubted Trenton age, that we can scarcely doubt the correctness of our interpretations of them. Forms 3 *g–i*, which are quite common, proved extremely puzzling, and have been referred as above only after much laborious comparison.

In the same layers affording the fossils just mentioned, there occur other and larger forms, which appear to us to be likewise crinoidean, with a strong probability that they represent the remarkable species found so abundantly in the Trenton at Newburgh, N. Y., known as *Cleiocrinus magnificus* Billings.* Transverse sections of several of the supposed columns of this species are shown in figs. 4 *e–i* inclusive, and portions of several columns, an inch or more in length, are shown in figs. 4 *a–d*. The absence in these forms, of the minute details of structure so admirably exhibited in the Newburgh specimens, forbids our reaching entirely satisfactory conclusions as to their identity; but the general form of the Canaan specimens, their size, thickness of walls, more or less crystalline, radiating structure, as shown in the transverse section (fig. 4 *e*) so characteristic of crinoidal columns; their surface markings which, upon such weathered specimens as represented by figs. 4 *a*, *b* and *d* seem indicative of close transverse joints, and the evident occurrence of encrinal columns of large size associated with the smaller ones, as in fig. 2, all point toward such identity. Some of the columns



Crinoids from Newburgh, N. Y., natural size.—Nos. 1, 2 and 4, *Cleiocrinus magnificus* (Billings). No. 1, end view of a specimen which is rounded by weathering, and thickened within by incrustation. No. 2 is somewhat compressed in a vertical plane. No. 4 is broken open and very much flattened. No. 3. *Cleiocrinus grandis* (Billings), end view of a very perfect specimen, giving all the characteristic features.

* Geological Survey of Canada, Decade IV, page 54, Plate 5, fig. 3.

of *C. magnificus* collected at Newburgh are flattened so as to present a transverse section precisely like that shown in fig. 4 *i*. For the sake of comparison we have introduced in the text figures illustrative of the columns of *C. magnificus* and *C. grandis* from the Newburgh locality. Fig. 5 represents a portion of the column of another species probably of the same genus, which is chiefly remarkable for the slenderness of its transverse joints. Of these there appear to have been not far from 112 in the space of an inch. The specimen was obtained by grinding down the surface of a small fragment of the Canaan limestone, which abounds in other organisms. It is possible that this fragment may represent a new species of *Cleiocrinus*, and should it turn out to be so, we propose to call it *Cleiocrinus Billingsi*.

Associated with the forms above described there occur several others (figs. 6 *a–g*) all of which may possibly represent portions of large encrinal columns; but as we do not feel satisfied that they are organisms of this nature, and are unable, at the present moment, to refer them with certainty to the Cephalopoda or any other class, we have decided to leave the question of their exact relations an open one. If Cephalopoda, the apparent absence, in all of the examples, of undoubted septa is noteworthy and remarkable. It is barely possible that the oblique lines seen in the interior of fig. 6 *a*, and those crossing 6 *e* may indicate septa, but we cannot feel sure upon this point. In one example (fig. 6 *f*) there are faint indications of the presence of a siphuncle. Some of the specimens, as for example, figs. 6 *c* and 6 *d* suggest a comparison with the opercula of *Maclurea magna;* and we herewith gladly express our obligations to Prof. H. M. Seely, of Middlebury College, Vermont, for his kindness in generously supplying us with beautiful specimens of such opercula for comparison during our investigations.

In the specimen represented by figs. 7 *a*, *b*, there are shown upon one side (fig. 7 *a*) certain more or less definite markings, which very possibly indicate original structure; but we are in doubt as to whether such is really the case. If the markings noticed really represent original structure, they suggest a comparison of the form with specimens of the opercula of *Maclurea magna* now before us. It is quite possible, however, that the specimen is only the column of a *Cleiocrinus*, or rather a portion of one, in which event the markings spoken of might represent the edges of distorted disks.

Bryozoa.—The only specimen referable to this class that we have recognized among the Canaan fossils, is that shown in figs. 8 *a*, *b*. It was discovered upon the surface of a freshly broken fragment of dark colored, richly fossiliferous limestone, and is very well defined. A careful study of it has led us to

the conclusion that it is most probably a species of *Fenestella* or some closely related generic form; but its specific determination has not been found possible. The specimen is principally interesting as showing that very delicate organic structures have been preserved in a limestone which is, for the most part, so highly metamorphic, that nearly every vestige of fossils has been obliterated; and further, as an indication that, at some favored locality in this limestone, a large and well-preserved suite of decisive specimens will yet be obtained.

BRACHIOPODA.—Fig. 9 appears to us to represent, most probably, the cross section of a brachiopod of about the size and form of the well-known Trenton species *Orthis occidentalis*. It was discovered by polishing a surface of the dark Canaan limestone, and is associated with other organisms. Similar, though less convincing sections, on which we have put the same interpretation, have been observed in other slices which we have studied, and are probably of the same nature. Apart from these examples, however, no forms certainly referable to the Brachiopoda have been obtained from the Canaan limestones, unless some of the structures exhibited in figs. 3 *f*, *i*, represent sections of the ribs of small *Orthidæ*, or allied forms, which may possibly be the case.

GASTEROPODA.—The fossils of this class furnished by the Canaan limestones, afford the most decisive testimony concerning the age of the rocks in question of any that we have examined. They occur in considerable numbers in certain portions of this limestone, associated with comminuted crinoidal remains and other organisms, and are well shown both in transparent rock-sections and upon carefully polished surfaces of ordinary hand-specimens (see fig. 1). We refer the majority of the forms met with to two species, one of which (figs. 10 *a–i*) appears to be, with but little doubt, the *Pleurotomaria subtilistriata* of Hall (Pal. N. Y., vol. i, p. 172, pl. 37, figs. 5 *a–d*); and the other (figs. 11 *a–f*), with even less doubt, the well-known Trenton species *Murchisonia gracilis* (Pal. N. Y., vol. i, p. 181, pl. 39, figs. 4 *a–c*; *ibid*, p. 303, pl. 83, figs. 1 *a–c*). Professor Hall states, in his description of *P. subtilistriata*, that the majority of the specimens are so small as to be scarcely visible to the naked eye; and we have found this true of the Caanan species. This species appears abundantly in sections of the Rochdale Trenton, and slices from the two localities, placed side by side, apart from the apparent absence in the Canaan rock of the common Rochdale coral *Solenopora* (*Chætetes*) *compacta*, are practically undistinguishable.

Associated with the above species there occur the forms shown in figs. 12, 13, 14 and 15, which we look upon as probably representing other species of Gasteropoda, but are unable

to make any sure assignment of them. Fig. 13 is quite possibly a partial transverse section of a shell belonging to the group above referred to *Pleurotomaria subtilistriata* but is not sufficiently definite to enable us to decide this point. Forms 14 and 15 we have simply figured as they appear in polished sections, but prefer for the present, to make no definite disposition of them. They seem to be either distorted forms, or accidental combinations of independent structures.

CRUSTACEA.—The single fragment of the free cheek of a trilobite shown in fig. 16, is the only fossil of this class thus far obtained from the Canaan limestones. We think that it may represent the genal spine of some such species as *Asaphus megistos.*

Fig. 17 apparently represents a coral, which we think, is likely to be the *Chætetes lycoperdon.* It is a portion of a transparent rock section taken from limestone collected by Professor Dana upon Hall's farm above mentioned. We have been led to suspect the presence of corals, very much altered, among the organisms from the other localities; but have found nothing as definitely outlined as the specimen figured, that can be safely regarded as of this nature. In several of the slices we have also detected what appears to us to be a Stromatoporoid form.

Concluding Remarks.

Judging from the facts above presented, we strongly incline to the opinion that the limestones of Canaan, N. Y., which have afforded the fossils in question, are of Trenton age. We are unable to assert positively that the three Trenton species *Cleiocrinus magnificus, Pleurotomaria subtilistriata,* and *Murchisonia gracilis* have been identified among the organisms obtained from these limestones; but we neverthless feel very confident that the forms which we have recognized as probably such, will turn out to be what we have considered them. We consider it safe to affirm that one species, the *Murchisonia gracilis,* has been recognized with so much probability, that its identification may be regarded as established with as much certainty as is possible from a study of sections alone; and we feel almost equally confident in regard to the *Pleurotomaria subtilistriata.* It should be remembered that the more decisive forms brought to our notice, have been presented to us, with but few exceptions, either in polished surfaces or transparent rock-sections; and that the difficulties in the way of correct determination have been, in consequence of this, greatly increased, as compared with determinations undertaken in the usual manner. We present this brief paper hoping that it may serve to throw additional light upon a much debated region, and lead to the development of still more convincing paleontological testimony concerning the precise age of its strata.

EXPLANATION OF PLATE VII.—*Fossils from Canaan, N. Y.*

All the figures are of the natural size except where otherwise described.

Fig. 1.—A transparent section, from the upper dark colored layers at tunnel No. 192; containing abundant fragments of crinoidal stalks, and a number of small gasteropods, probably *Pleurotomaria subtilistriata.*

Fig. 2.—A polished surface of a specimen from the darker layers at the tunnel, filled with sharply defined fragments of small crinoidal columns; containing also several pieces of joints of large crinoids of the *Cleiocrinus* type.

Fig. 3*a*.—A sharply defined vertical section of a crinoid stalk, apparently of the genus *Heterocrinus;* from a transparent slice of a specimen from the tunnel. 3*b*. Same as 3*a*, but twice the natural size. 3*c–g*. Joints of crinoid stalks, all transparent sections: *c* and *d*, are from the locality 2400 feet southerly from the tunnel; the remainder are from the dark layers of the tunnel. 3 *h–j*, doubtful organisms in transparent sections from the tunnel; probably crinoidal fragments (compare with 3*g*), though possibly oblique sections of brachiopod shells. Figs. 3, *e–j*, are twice the natural size.

Figs. 4*a–d*.—Large organisms in relief upon weathered surface of the lower lighter colored layers of limestone at the tunnel. Probably large encrinal stalks of the *Cleiocrinus* type. 4*e–i*. Horizontal sections, probably of large crinoidal stalks of the *Cleiocrinus* type; these appear conspicuously on the surface of the lighter-colored layers at the tunnel. The two separate portions of specimens 4*g* and *f* are on opposite sides of their respective fragments, and are separated by about a quarter of an inch of rock.

Fig. 5.—Portion of a crinoidal stalk, apparently of the genus *Cleiocrinus*, with about 112 joints to the inch; proposed specific name, *Billingsi*. On a polished surface, from upper layers, at the tunnel.

Figs. 6*a–g*.—Various conspicuous, but very doubtful organisms from the lower and lighter layers at the tunnel; presenting features of crinoids, but some also suggesting *Orthocerata*. 6*a*. Shows possible indications of septa. 6*f*. Shows a possible siphuncle; a cross-section of each extremity is given. 6*g*. A cross-section of the smaller extremity, so far as preserved, is given.

Figs. 7*a* and *b*.—The opposite sides of a doubtful organism from the weathered surface of the lower layers at the tunnel. 7*a*. Showing the polished surface of the shell on that side, exhibits what may be lines of structure, about 70 to the inch, variously curved.

Fig. 8*a*.—*Fenestella?* sp.? From dark layers at the tunnel. 8*b*. Same, × 2.

Fig. 9.—Apparently a horizontal section through the hinge of a large brachiopod, like *Orthis occidentalis*. On a polished surface, from the tunnel.

Figs. 10*a–i*.—*Pleurotomaria subtilistriata* probably; *b* and *e* are twice the natural size, and are taken from a transparent slice from the upper layers at the tunnel; *d* is from another transparent section from the locality 2400 feet southerly from the tunnel. *a*, *c*, *f*, *h* and *i*, are all from a polished surface of a single small specimen from the tunnel, upper layers. *g* is from another polished surface.

Figs. 11 *a–f*.—*Murchisonia*, probably *M. gracilis*, *a*, *b* and *d*, are all exhibited on polished surfaces from the tunnel, upper layers, *a* and *d* occurring in the same specimen with the *Pleurotomarias a*, *c*, *f*, *h* and *i*. 11*e* and *f* are the more prominent ones in a confused group of *Murchisonia*, seen in a transparent section from the locality south of the tunnel.

Fig. 12.—Partial vertical and horizontal sections through a single gasteropod (*Pleurotomaria?*) from the tunnel.

Fig. 13.—A partial vertical section passing nearly through the center of a gasteropod, exhibited on the polished surface of the specimen from the tunnel containing the *Pleurotomarias*, *a*, *c*, *f*, *h* and *i*.

Fig. 14.—Probably fragments of one or more gasteropods; polished section from tunnel.

Fig. 15.—Doubtful specimen from the same polished surface as fig. 13.

Fig. 16.—Genal spine of trilobite, perhaps *Asaphus megistos*. From upper layers at tunnel. Exhibited naturally on a freshly fractured surface, being itself nearly black.

Fig. 17.—Apparently a fragment of a coral, perhaps "*Chætetes lycoperdon.*" In a transparent slice of a hand-specimen of limestone collected by Prof. J. D. Dana on Hall's farm, Canaan, N. Y.

ART. XXIV. — *On Surface Transmission of Electrical Discharges;** by H. S. CARHART.

THE Report of the Commissioner of Patents for 1859, volume "Agriculture," contains an important but little known contribution by Professor Joseph Henry on "Atmospheric Electricity." Under the head of "Electricity in Motion" occurs this passage:—"If the discharge be not very large in proportion to the size of the conductor, it will principally be transmitted at the surface. If the charge be very large, and the conductor small, it will probably pervade the whole capacity, and, as we have seen, will convert into an impalpable powder or vapor the solid particles." Of the latter statement abundant demonstration has recently come under the writer's notice To settle the former point the author of the paper alluded to instituted a series of experiments, only one of which is described. It is fair to infer that this one was deemed the most conclusive. The arrangement in his own words was as follows: "C D is a copper wire . . . . . of the size usually employed for ringing door-bells, passing through the axis of an iron tube, or a piece of gas-pipe, about three feet long. The middle of this wire was surrounded with silk, and coiled into a magnetizing spiral, into which a large sewing needle was inserted. The wire was supported in the middle of the tube by passing it through a cork at each end, covered with tin-foil, so as to form a good metallic connection between the copper and the iron. F and Y are two other magnetizing spirals of iron wire, on opposite sides of the tube, the ends soldered to the iron. When these two spirals were also furnished with needles and a discharge from a Leyden jar sent through the apparatus, as if to pass along the wire, the needle inside of the iron tube was found to exhibit no signs of magnetism, while those on the outside presented strong polarity. This result conclusively shows that, notwithstanding the interior copper wire of this compound conductor was composed of a material which offered less resistance to the passage of the charge than the iron of which the outer portion was formed, yet when it arrived at the tin-foil covering of the cork, it diverged to the surface of the tube, and still further diverged into the iron wire forming the outer spirals."

Several years since I repeated this experiment apparently with the result described above. Doubt was cast on the demonstration, however, by the discovery in one trial that the inner needle was slightly magnetized.

* Read before the American Association for the Advancement of Science, August 27, 1885.

It is evident that the experiment is not conclusive, for the arrangement described constitutes a divided circuit in which one branch, consisting of the gas-pipe, presents much less resistance than the interior copper wire. The greater part of the discharge should accordingly pass along the gas-pipe, and the magnetization of the interior needle would be so slight as to escape detection unless a delicate method of examination were employed.

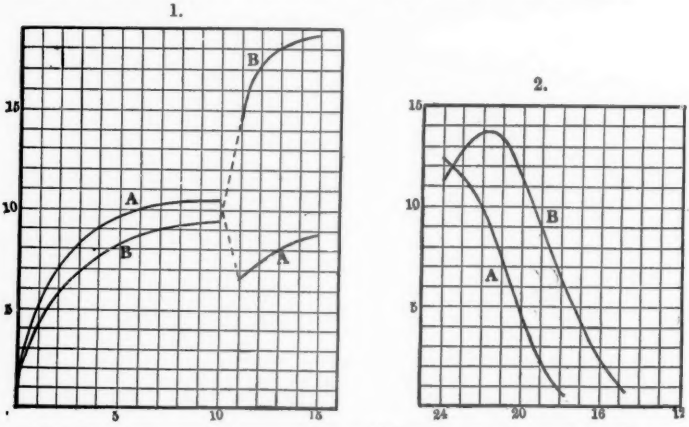
The matter seemed to be of sufficient importance to justify an experimental inquiry. The investigation has been more extended than was anticipated, because it has met with greater difficulties than were at first apparent; but the results obtained are of considerable interest, and the conclusion of the whole matter settles the main question.

The following arrangement of apparatus was adopted. Two glass tubes about 15^{mm} in diameter were covered with tin-foil for a length of about one meter each. About the tin-foil at each end was secured tightly a copper clamp. Two similar magnetizing spirals were made by winding twenty turns of gutta-percha-covered wire round small glass tubes. One of the spirals served to connect the two tin-foil coatings in series by having its terminals soldered to the copper clamps. The other formed the middle of a circuit extending through the inside of the tubes. The two conductors were then joined at the remote ends of the glass tubes so as to form a divided circuit,—one of the tin-foil and connecting helix, the other of the wire passing through the inside of the tubes and containing the other helix. *The two branches were adjusted to very accurate equality of resistance.* Through this compound circuit a Leyden jar was discharged; and the method finally employed to measure the relative portions of the charge passing through the two branches was to determine the magnetic moment imparted to two similar rods of steel, $1{\cdot}8^{mm}$ in diameter and 6^{cm} long, tempered glass-hard, one inserted in each spiral. The Leyden jar of about 700 square centimeters surface was charged by a Holtz machine with fifteen sparks from two to three centimeters in length. The magnet in the inner branch of the divided circuit will be called A; the one in the outer, B.

What Hughes calls "the evident magnetism" of A was in every case greater than that of B after a single discharge of the jar. Upon sending repeated discharges through the circuit, the ratio of the two magnetic moments gradually diminished and the moments themselves approached a maximum, as is shown in fig. 1, where the ordinates denote magnetic moments and the abscissas discharges of the Leyden jar. The moments in arbitrary units are simply fractional parts of the deflection

of the magnetometer, consisting of three pieces of magnetized watch spring attached to the back of a plane mirror. This small magnet was about one centimenter long; and, with the attached mirror, was suspended by a cocoon fiber 10^{cm} long, after the manner described by Gray.* The deflections were read by a telescope and scale at a distance of $1{\cdot}8^{m}$, though this distance varied somewhat in the different sets of experiments, inasmuch as the only quantitative comparisons made were between each pair of magnets used in the magnetizing spirals. The maximum moments were usually attained at ten discharges, the magnetization of the two rods being then pretty nearly equal.

After this nearly stationary condition had been reached, some unexplained facts led to an exchange of the two magnets in the spirals, keeping them turned so that a subsequent discharge would still magnetize them in the same sense as before. A single discharge of the Leyden jar then sufficed to increase the magnetic moment of B from 50 to 60 per cent, and to diminish that of A about half as much, as shown in fig. 1. This was repeated many times with the same invariable result. Continuing the discharges the ordinates of both curves increased as shown. After the magnetization of both rods had approached an apparent maximum, therefore, the mere exchange of the magnets in the spirals was followed by this striking result.



Not finding any information guiding me to an explanation of this curious behavior of the magnets, I sought it by an examination of the physical condition of the magnets themselves after they had reached the maximum of magnetic moment with ten discharges. The outside was gradually eaten away with

* "Absolute Measurements in Electricity and Magnetism," p. 6.

nitric acid, the magnets weighed after each immersion in the acid, and their magnetic moments determined. The results of this process with one pair of magnets are plotted in fig. 2, in which the ordinates are magnetic moments and the abscissas are weights in units of fifty milligrams. The curves therefore represent magnetic moments decreasing with weights. It will be observed that the moment of A decreases from the first continuously, while that of B first rises to a maximum and then falls according to the law exhibited by A. These curves afford a complete explanation of the anomalous deportment of the magnets upon exchanging them in the coils. B has a thin external shell magnetized in a sense opposite to that of the underlying portions. When B takes A's place, this external magnetism is reversed by the first subsequent discharge and made similar to the underlying portions; while A, being placed under B's former conditions, suffers a reversal on the exterior, thus losing in resultant magnetic moment, or "evident magnetism." When the magnets are examined after a single discharge of the Leyden jar they give rise to exactly similar curves, except that the reversed portion of curve B is somewhat longer.

An examination of the conditions under which an oscillatory discharge takes place, and particularly of the formula for the period of an oscillation, will throw light on the reversed curve of B. The first mathematical discussion of electrical oscillations was by Sir William Thomson in 1853* and was founded on the "equation of energy." Kirchhoff† subsequently arrived at similar equations by introducing into the expression for the electromotive force a coefficient of self-induction. Later writers, Chrystal,‡ Mascart and Joubert,§ have followed the method of Kirchhoff. These investigations show that the discharge will be oscillatory when $R<\sqrt{\frac{4L}{C}}$, in which R is the resistance of the discharger, L the coefficient of self-induction, and C the capacity of the principal conductor. The period of an oscillation is $\frac{\pi}{n}$ where $n=\sqrt{\frac{1}{LC}-\frac{R^2}{4L^2}}$. When R is very small, the period of an oscillation is proportional to $\sqrt{LC}$. In all my experiments the greatest value of R, not including the air resistance of the spark, was 0·58 ohm. We may, therefore, for our purposes consider the period of an oscillation jointly proportional to the square root of the ca-

* Mathematical and Physical Papers, vol. i, p. 540.

† Gesammelte Abhandlungen, "Zur Theorie der Entladung einer Leydener Flasche," p. 168.

‡ Encyclopædia Britannica, vol. viii, p. 81.

§ Electricity and Magnetism, vol. i, p. 512, English edition.

pacity of the principal conductor and of the coefficient of self-induction. This inference, so far as it relates to the capacity of the principal conductor, was confirmed by some experiments of Helmholtz in 1869.*

The bearing of the duration of an oscillation on the reversal of the superficial magnetism is that the mean intensity of the current during the first period is inversely proportional to the length of the period, since the whole quantity of electricity on the principal conductor is discharged during this short interval. Then the magnetic moments, being nearly proportional to the current strength, so long as the saturation point is not approached, will therefore vary inversely as the periods of an oscillation, or inversely as the square root of the coefficients of self-induction, in the two branches of the discharger, even though the quantity discharged through the two branches is the same. The magnetic impulse of both the direct and reverse oscillatory discharge is greater the smaller the coefficient of self-induction in the discharger.

In the experiments thus far described no care was taken to make the coefficient of self-induction in the two branches equal to each other. The inner branch was of copper wire longer than the glass tubes, and coiled into a long helix with the consecutive turns far enough apart to prevent a spark from leaping across. This disposition of the wire increased largely its self-induction. The time of an oscillation through it was therefore greater than in the outer branch, and the magnetic effect of the weaker current less, while the whole quantity discharged through the two branches was the same. Not only then was the mean magnetic moment of B greater than that of A, but the effect of the return discharge was sufficient to produce a reversal of magnetism in B near the surface.

These conclusions are fully confirmed by experiments made with a new arrangement of apparatus, so devised that the coefficients of self-induction in the two branches should be as nearly equal as possible; while at the same time their capacities were made more nearly equal than before, in order to eliminate any possible effect on the period of an oscillation arising from dissimilarity in this respect.

Two† large tubes, 22^{mm} in diameter, were covered with tin-foil for a combined length of 265^{cm}. Two other tubes 15^{mm} in diameter, were also covered for a combined length of 296^{cm}, and the smaller tubes were placed inside the larger ones. One of the magnetizing spirals was made to connect the outer tubes in series, and the other, the inner ones, the other ends of the tin-foil conductors being joined by means of copper clamps and

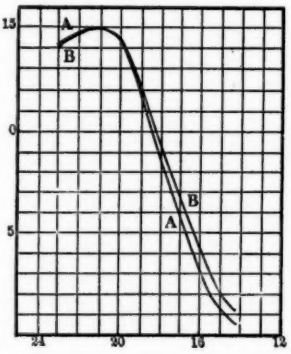
* Wissenschaftliche Abhandlungen, "Ueber Elektrische Oscillationen," p. 531.

† These experiments were made after the reading of the paper at Ann Arbor.

short pieces of wire, so as to make a divided circuit of two branches as before. The cross-section of the tin-foil conductor on the inner tube was made as nearly as possible equal to that of the outer, so that equality of resistances was secured without introducing materially different lengths of wire into the two branches. The resistances in every case were adjusted to equality by means of a sensitive mirror galvanometer and a Kohlrausch bridge.

With this apparatus the magnetic moments of two magnets of equal weight, cut from the same steel rod and tempered together, are almost exactly equal; and the curious phenomenon exhibited in fig. 1, when the magnets exchanged places in the two coils, very nearly disappears. What is perhaps more to the purpose, the lateral distribution of magnetism in the two magnets is practically identical. Two steel rods as before were placed in the magnetizing spirals; and after ten discharges of the Leyden jar, an examination was made by gradually removing the outer shell with nitric acid and determining the magnetic moment after each removal of about 50^{mgs}. The two resulting curves, shown in fig. 3, are nearly identical. Both show the same superficial reversal of magnetic polarity. In fact, if curve B is rotated clock-wise through a small angle round its highest point it will coincide with that of A. The depth to which this reversed magnetism extends depends, therefore, almost entirely on the coefficient of self-induction of the discharger, the principal conductor of definite capacity being charged to a given potential. When the only dissimilarity between the two branches of a divided conductor is that one is placed inside the other, there is no dissimilarity in the quantity or distribution of the magnetism produced in similar coils in the two branches by a discharge through the compound circuit. There is no evidence of a tendency toward surface transmission. The law of resistance pertaining to dynamic currents applies also to electric discharges. The resistance is a function of the area of the cross-section and not of the periphery of the cross-section.

3.



A practical bearing of the investigation is that there is no sufficient scientific basis for making lightning conductors of large surface; and that large sectional area in essential to ample conductivity.

Evanston, Ill.

ART. XXV.—*The Minerals of Litchfield, Maine;* by F. W. CLARKE.

IN Kennebec County, Maine, along and near the boundary between the towns of Litchfield and West Gardiner, are scattered many bowlders of an elæolite rock. For many years these have yielded to collectors of minerals superb specimens of blue sodalite, yellow cancrinite and zircon; but although the parent ledge appears at several points, it seems nowhere to have been opened. In addition to the minerals already mentioned, the bowlders contain albite, lepidomelane, a black mineral resembling columbite, a flesh-colored mineral which has been called indiscriminately elæolite or cancrinite, and a massive alteration product known to local collectors under the provisional name of "white sodalite." Although specimens from the locality are widely distributed in cabinets, some of the minerals seem to have been but partially described; and I have therefore thought it worth while to study them somewhat closely. The supposed columbite I have not examined, for want of material; the zircon I have omitted, since it has been sufficiently studied by Gibbs;* but the sodalite and cancrinite, although they had been well analyzed by Whitney,† I have included in my investigation, for reasons which will appear below.

ELÆOLITE.

This species occurs abundantly in Litchfield and West Gardiner in characteristic, dark gray, cleavable masses of strong greasy luster. Since it is the typical mineral of its group, and as I can find no published analysis of it from this locality, the following results may have value as a matter of record.

H_2O	·86
SiO_2	43·74
Al_2O_3	34·48
CaO	*trace*
MgO	*trace*
K_2O	4·55
Na_2O	16·62
	100·25

The specimen analyzed contained minute inclusions of black mica, but not enough of them to notably affect its composition. The analysis agrees fairly well with the published analyses of elæolite from other places.

* Poggend. Annalen, lxxi, 559. † Poggend. Annalen, lxx, 431.

Cancrinite.

This mineral is one of the most abundant and characteristic at the locality, and varies considerably in appearance. Two analyses of it were made by Whitney, one of the yellow variety, the other of a greenish modification. I have myself seen nothing to answer to the latter description, but have selected three typical samples for investigation. They may be briefly described and indicated as follows:

A. Bright orange yellow, with strong luster and cleavage, transparent in thin fragments.
B. Dirty pale yellow, less lustrous, highly cleavable, also transparent in thin fragments.
C. Bright yellow, granular. The commonest variety.

For ease of comparison I have tabulated the analyses side by side with Whitney's; indicating his yellow cancrinite by "D," and his greenish variety by "E." The carbonic acid determinations were made for me by Mr. R. B. Riggs, who used the Gooch tubulated crucible, and collected the gas evolved directly in a potash bulb.

	A.	B.	C.	D.	E.
SiO_2	36·29	35·83	37·22	37·42	37·20
Al_2O_3	30·12	29·45	28·32	27·70	27·59
Mn_2O_3	*trace*	*trace*	*trace*	·86	·27
Fe_2O_3	"	"	"		
CaO	4·27	5·12	4·40	3·91	5·26
Na_2O	19·56	19·33	19·43	20·98	20·46
K_2O	·18	·09	·18	·67	·55
MgO	----	----	·07	----	----
H_2O	2·98	3·79	3·86	2·82	3·28
CO_2	6·96	6·50	6·22	5·95	5·92
	100·36	100·11	99·70	100·31	100·53

It will at once be observed that cancrinite "A," which, from its appearance, was presumably the purest type of the mineral, is the highest of all in carbonic acid and lowest in water. It is also the highest in soda and alumina. Whitney's two analyses show more potash than mine, but in other respects run fairly near "C," which, as I have said, represents the commonest, and probably the least pure variety. But in order to understand the variations better, we must consider the flesh-colored mineral referred to in my introductory paragraph, which, as I have said, has been called indiscriminately elæolite or cancrinite, according to the fancy of the collector. It sometimes occurs in specimens of considerable size, is lustrous and cleavable, and to the eye appears perfectly homogeneous. An analysis gave the following results; the carbonic acid, as in the other cases, being determined by Mr. Riggs.

SiO_2	38·93
Al_2O_3	32·52
CaO	2·47
Na_2O	17·02
K_2O	3·23
H_2O	2·83
CO_2	2·95
	99·95

These figures plainly indicate that the mineral is a mixture of elæolite and cancrinite, but do not show whether the mixture is mechanical, or due to isomorphism. To determine this point, Mr. J. S. Diller kindly undertook a microscopic examination of the material, comparing it in thin sections with the elæolite and cancrinite "B," from the specimens of which portions were previously analyzed. He found the mineral to be a merely mechanical commingling of the two species, in nearly equal proportions, and later he succeeded in separating them by means of Sonstadt's solution. This fact, considered together with the apparent homogeneity of the material, renders it probable that the variations in composition of the cancrinite are due to small admixtures of elæolite; and that Whitney's specimens were rather more so contaminated than mine. Still, the entire series of cancrinite analyses are fairly concordant, and confirmatory of each other. In discussing the formula of the mineral, however, analysis "A" will be given preference.

Sodalite.

On account of its beauty, and its intense blue color, this mineral, as it occurs at Litchfield, is a favorite among collectors. It is now somewhat scarce, at least in large or compact specimens, and it ought to be carefully searched for in place. It often occurs intermingled with cancrinite, forming beautifully mottled masses, and also is associated intimately with the white, massive alteration-product to be described later. The following analysis was made, partly for comparison with Whitney's, and partly to aid in the study of the accompanying white mineral.

	Clarke.	Whitney.	Whitney.
SiO_2	37·33	37·30	37·63
Al_2O_3	31·87	32·88	30·93
Fe_2O_3	----		1·08
Na_2O	24·56	23·86	25·48
K_2O	·10	·59	*undet.*
Cl	6·83	6·97	*undet.*
H_2O	1·07	----	----
	101·76	101·60	
Deduct $O=Cl$	1·54		
	100·22		

In my analysis iron was not looked for, because the ignited alumina, which should have contained it if present, was perfectly white. Otherwise the analyses agree tolerably well.

Hydronephelite, a New Species.

Intimately associated with the sodalite is the white alteration product mentioned in the last paragraph. So close is the association, in fact, and so similar in occurrence are the two minerals, that the latter has been called white sodalite by the local collectors. Like the sodalite it is found in seams, and yields specimens as much as two centimeters in thickness; it is white, lusterless, and has the fracture of sodalite; and probably it originated from the alteration of the latter. Two specimens of it were analyzed, which were received from two different collectors, with the following results:

	A.	B.
H_2O	13·12	13·30
SiO_2	38·90	39·24
Al_2O_3	33·98	33·16
CaO	·05	*trace*
Na_2O	13·21	13·07
K_2O	1·01	·88
Cl	*trace*	----
	100·27	99·65

The alumina carried a trace of iron, and a doubtful trace of manganese was also indicated. Hardness, 4·5. Fusible easily to a white enamel. Soluble in hydrochloric acid, and gelatinizing upon evaporation. Fracture irregular, resembling that of the sodalite. In general, the mineral may be said to have the appearance of a slightly altered feldspar, minus the distinct cleavage.

These analyses left little doubt in my mind that I had a new mineral to deal with, and one belonging to the zeolite family. Such minerals are well-known derivatives of the nephelite group, and thomsonite and natrolite have especially been often noted. In composition the new product differs distinctly from natrolite, but agrees in ratios approximately with thomsonite; forming, as far as chemical evidence alone goes, the soda end of a series passing through rauite, up to ozarkite; the last named mineral being the nearest towards the lime end of the series. A comparison of the analyses of these elæolite derivatives is worth making, on account of its suggestiveness. The ozarkite was analyzed by Smith and Brush, the rauite, from Brevig, by Paykull.*

* Ber. der Deutsch. Chem. Gesell., vii, 1334.

	Ozarkite.	Rauite.	Hydronephelite.
H_2O	13·80	11·71	13·12
SiO_2	36·85	39·21	38·90
Al_2O_3	29·42	31·79	33·98
Fe_2O_3	1·55	·57	----
CaO	13·95	5·07	.05
Na_2O	3·91	11·55	13·21
K_2O	----	----	1·01
	99·48	99·90	100·27

Inasmuch, however, as massive minerals, and especially those which are produced by processes of alteration, are always subject to doubt, I requested Mr. Diller to assist me with a microscopic examination of the new substance. He very kindly acceded to my request, and I subjoin an abstract of his results.

"A section was carefully prepared so as to show both the sodalite and the white, lusterless mineral associated with it, in such a way as to reveal their relations. The extremely irregular line of contact between the sodalite and its secondary products is well defined in transmitted light, but is even more distinct between crossed nicols from the fact that the sodalite, being isotropic, remains dark in all positions, while the other minerals are more or less brilliantly colored. The secondary products, which have clearly resulted from the zeolitization of the sodalite, are two in number. One of them forms very much the larger portion, probably nearly 90 per cent of their total amount, and the other is imbedded in the first in the form of distinct grains. Under the microscope in transmitted light the predominating mineral, which is doubtless a zeolite as shown by your analyses, is more or less deeply clouded like decomposed feldspar. Between crossed nicols it breaks up into flaky grains which vary considerably in the intensity of their color. Some remain dark, others range through light and medium tints of red and yellow, according to the position of the section. The isotropic grains in converging light are proved to be distinctly uniaxial and positive, and the anisotropic ones as far as can be determined exhibit parallel extinction. It is evident therefore that the zeolite must be either quadratic or hexagonal in the system of its crystallization. Some of the grains show an indistinct striation approximately parallel to the vertical axis, but a distinct cleavage could not be discerned. In basal sections three sets of fractures could be rarely made out with sufficient distinctness to suggest that the mineral is probably hexagonal. The mode of its occurrence indicates clearly that it has resulted from the zeolitization of the sodalite; a phenomenon which has been observed in many rocks.

"The small grains of the other secondary mineral are so intermingled with the uniaxial zeolite as to indicate that both are derived from the sodalite. They are easily distinguished from the zeolite in which they are imbedded. In transmitted light they are perfectly clear and transparent, with so high an index of refraction as to appear to rise above the surrounding mass. The grains are entirely without crystallographic boundaries, but are traversed by distinct cleavage lines. Between crossed nicols they are much more brilliantly colored than the associated zeolite, and if the section is rotated they become dark when the cleavage lines make a prominent angle (15°–33°) with the principal sections of the prisms. The mineral is certainly biaxial, and in all probability belongs to one of the two inclined systems of crystallization, but its definite determination is not practicable under the circumstances."

In view of the presence of an impurity in the new zeolite, Mr. Diller suggested a re-analysis of it, to be made on carefully purified material. The purification, by means of Sonstadt's solution, he kindly undertook, determining at the same time the specific gravity of the mineral. The crude material gave him a sp. gr. of 2·263, while the zeolite was a little lighter and the imbedded grains a little heavier. After purification the coarsely powdered zeolite was carefully picked over under the microscope until Mr. Diller felt confident that the sum of all impurities could not exceed one per cent. The mineral, then dried at 100°, gave me the following analytical results:

H_2O	12·98
SiO_2	38·99
Al_2O_3	33·62
CaO	·07
Na_2O	13·07
K_2O	1·12
	99·85

These figures confirm the previous analyses, and show that the impurity which vitiated them must have been small in amount and similar in composition to the new zeolite. The latter, I think, may be considered as fairly well established; and its formula may be written $Al_2(SiO_4)_3Na_2H.3H_2O$; which requires, water, 13·76; soda, 13·54; alumina, 33·41, and silica, 39·29. This composition, and the manifest relations of the mineral to nephelite, the parent member of the group, naturally suggest for it the name *hydronephelite*, which seems to be both appropriate and descriptive. Chemically, as I have already observed, the species approximates to a soda thomsonite; but optically it appears to be quite different. This fact suggests the desirability of a careful microscopic re-examination of all

the other massive zeolitic alterations of elæolite which have been on analytical grounds referred to the thomsonite series. Hydronephelite, indeed, is directly derived from sodalite, but the latter itself probably originated from elæolite; so that the new species may quite properly be considered along with the other zeolites which were previously mentioned. The fact that it contains more potassium than the sodalite, is noteworthy, and calls for an explanation which I am unfortunately not prepared to offer.

Albite and Lepidomelane.

The albite of Litchfield, which appears to be associated with other undetermined feldspars, is mostly in obscure masses. Occasionally a fragment is found with a translucent cleavage surface one or two centimeters broad. Such a specimen was partially analyzed, giving H_2O 0·52, SiO_2 66·39, Al_2O_3 19·69, K_2O 0·99, Na_2O 10·17. These figures serve only for complete identification of the species.

The lepidomelane exists abundantly in the elæolite rock, but mostly in small black scales. Sometimes tolerably large plates of it are found, black and brilliant, decidedly brittle, and apparently affected by alteration. An analysis gave the following results. The iron determinations were made by Mr. Riggs.

H_2O	4·62
F	none
TiO_2	"
SiO_2	32·09
Al_2O_3	18·52
Fe_2O_3	19·49
FeO	14·10
MnO	1·42
MgO	1·01
K_2O	8·12
Na_2O	1·55
	100·92

This analysis is noteworthy on account of the extremely low percentage of silica, which is approached, so far as I can ascertain, only in an analysis by Rammelsberg of a black mica from Brevig. The ratio between silicon and oxygen is nearly 1 : 5, which agrees with no known formula. My results make it extremely probable that the mica is a mixture and that it has undergone an alteration tending toward the ultimate development of some chloritic species. Still it deserves, as also do the feldspars of the locality, a more thorough examination.

In attempting to discuss the formulæ of cancrinite, sodalite, and hydronephelite, certain points should be carefully borne in mind. First, the three species must be considered, not independently, but relatively to each other; for all the evidence indicates for them a common origin. That origin is from the first member of the group, elæolite or nephelite; the empirical formula for which has been finally fixed by Rauff.* In partially rational form it may be written $Na_8Al_8(SiO_4)_7(SiO_3)_2$; ignoring the small replacement of sodium by potassium which has been shown by synthetic investigations to be non-essential. Not only does the mode of occurrence and association of the minerals point to community of origin, but the same conclusion is emphasized by the experiments of Lemberg† upon the artificial alteration of silicates. When elæolite from Fredriksvärn was digested 180 hours with a solution of sodium carbonate, a partial transformation into a *soda* cancrinite was effected; while a digestion of six months with a caustic soda solution containing sodium chloride gave a product identical in composition with sodalite. Many such experiments were tried by Lemberg, yielding a large class of similar results. His method of procedure probably did not give absolutely pure or definite compounds, and yet his researches furnish evidence of great value in discussing the chemical structure of many minerals.

If we compare the published analyses of cancrinite from different localites, we shall find that they vary in two ways. First, there are variations which are probably due to small admixtures of elæolite, such as I have shown to occur at Litchfield; and secondly, the ratio between the lime and the carbonic acid ranges between rather wide limits. In the cancrinite from Miask, the two are about equivalent; while the Litchfield mineral contains only half enough calcium to saturate the carbonic acid. The lime and soda, however, vary reciprocally; so that when one is high, the other is low; and, furthermore, the experiment quoted from Lemberg goes to show that a cancrinite may exist containing no lime whatever. If this conclusion be correct, then the carbonic acid of the mineral must be represented as linked with aluminum; a supposition which finds some justification in the existence of the rare species dawsonite. The function of water in cancrinite remains doubtful; if it be regarded as water of crystallization, the formula of the residue becomes less easy to write intelligibly; but if it forms a part of the atomic structure, it is almost necessary to represent the carbonic acid as orthocarbonic, in the group CO_4. This mode of consideration, as will appear later, leads to a

* Zeitschr. für Kryst., ii, 445.

† Zeitschr. der Deutsch. Geol. Ges., xxxv, 557, 1883.

simple general formula for cancrinite, covering all variations in composition except such as are due to impurity, and correlating the mineral with the allied species sodalite and nosean. For the Litchfield mineral the following special formula may be written, giving the theoretical composition in the column below:

$$Al_8(SiO_4)_8(CO_4)_2CaNa_8H_6.$$

	Found.	Calculated.
SiO_2	35·83 to 37·22	35·9
Al_2O_3	28·32 to 30·12	30·6
Na_2O	19·33 to 19·56	18·6
CaO	4·27 to 5·12	4·2
CO_2	6·22 to 6·96	6·6
H_2O	2·98 to 3·86	4·1
		100·0

In this case the water as found is slightly lower, and the soda slightly higher, than the calculated values; which is probably ascribable to the mutual replaceability of sodium and hydrogen.

The formula commonly accepted for sodalite, and the one which is certainly the simplest, is that deduced by Bamberger* from his analysis of the mineral from Tiahuanuco. Written empirically, this formula is $Na_5Al_4(SiO_4)_4Cl$, which requires considerably less chlorine than has ordinarily been found in the species. In Bamberger's analysis, as finally corrected, he obtained 5·54 per cent, as against nearly seven per cent in Whitney's determinations. The difference he ascribes to silica in the chloride of silver as weighed by other analysts; and yet in my own estimation every care was taken to eliminate such impurity, and my results confirm the older figures. Still, both figures have theoretical interest, as will be seen further on; and I am inclined to believe that the Bolivian mineral was more nearly typical than that from Litchfield. To the latter we may assign the empirical formula $Na_9Al_7(SiO_4)_7Cl_2$, which is directly derivable from the formula for nephelite, and which agrees quite sharply with the analyses.

We now have three empirical formulæ ready for comparison side by side, as follows:

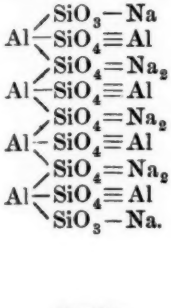
Nephelite	$Al_8(SiO_4)_7(SiO_3)_2Na_8$
Cancrinite (Litchfield)	$Al_8(SiO_4)_8(CO_4)_2CaNa_8H_6$
Sodalite "	$Al_7(SiO_4)_7Cl_2Na_9$

These may easily be put into structural form by an application of the principle suggested in a former paper,† that orthosilicates containing aluminum are to be represented as substi-

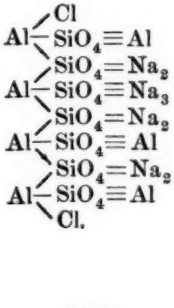
* Zeit. für Kryst., v, 581.

† Clarke and Diller, this Journal for May, 1885.

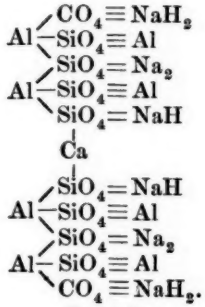
tution derivatives of the normal salt $Al_4(SiO_4)_3$. The latter contains the fundamental nucleus $Al(SiO_4)_3$, which appears to be capable of a sort of polymerization, and which forms the basis of the subjoined symbols.



Nephelite.



Sodalite.

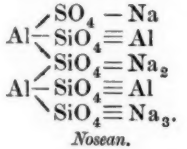


Cancrinite.

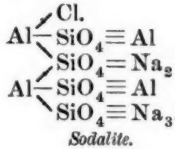
Now although these formulæ fit the analyses, and express a structural similarity of type, two of them are capable of a further generalization. Remembering the reciprocal variations between soda and lime in different cancrinites, and the fact that a soda cancrinite is quite possible, we may write the following general formula for that species; $Al_4(SiO_4)_4CO_4Na_5H_3$; which requires

SiO_2	35·8	
Al_2O_3	30·4	
Na_2O	23·1	Partly replaceable by CaO.
CO_2	6·6	
H_2O	4·1	

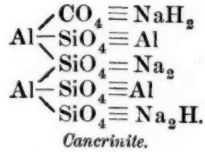
Comparing this with Bamberger's sodalite formula, and with the generally accepted formula for nosean, we have this remarkable series of structural expressions.



Nosean.

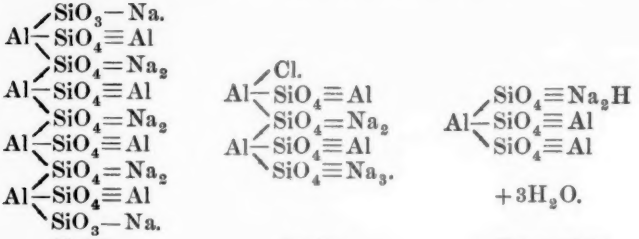


Sodalite.



Cancrinite.

The formula of hauynite of course reduces to the same type, and so also, probably, does that of microsommite. Hydronephelite has a still simpler formula, which, however, includes three molecules of water of crystallization. It may be advantageously compared in series with its parent minerals, in which case we have the following set of structures:



These formulæ express with decided clearness, the natural order of transition from one species to another. The alteration of a mineral necessarily involves the passage from a less stable to a more stable condition; and in this instance we observe precisely that state of affairs. From a quite complex and therefore easily disturbed molecule, through an intermediate, simpler compound, we pass to one which is simplest of all, and hence, presumably the most stable. I do not deny that such formulæ are subject to criticism, and that possibly the advance of knowledge may brush them to one side; and yet I feel justified in claiming that they have some real value in the coördination of observed facts, and that, through their singular suggestiveness, they assist in the prosecution of research.

Laboratory U. S. Geological Survey, Washington, 1886.

Art. XXVI.—*On the Chemical Behavior of Iron in the Magnetic Field;* by Edward L. Nichols, Ph.D.

[Read at the Ann Arbor meeting of the American Association for the Advancement of Science.]

When finely divided iron is placed in a magnetic field of considerable intensity and exposed to the action of an acid, the chemical reaction differs in several respects from that which occurs under ordinary circumstances. The cause of one such difference may be found in the fact that the solution of iron in the magnetic field is in a sense equivalent to its withdrawal by mechanical means to an infinite distance. Mechanical removal requires the expenditure of work and the same thing is doubtless true of what might be called its chemical removal. In other words the number of units of heat produced by the chemical reaction should differ, within and without the field, by an amount equivalent to the work necessary to withdraw the iron to a position of zero potential. Experiments upon this point which have been briefly described in a note already published

in abstract,* brought out other and unlooked-for modifications of the reactions.† These have been further investigated and are to be described in the present paper. My first experiments, the sole object of which was to detect differences in the heat of chemical action within and outside of the magnetic field, were conducted in the following manner. Between the poles of a small electro-magnet, with half inch core, was placed a beaker containing a known amount of acid. The temperature of the acid was indicated by a thermometer divided to fifths of a degree centigrade. A weighed quantity of powdered iron was introduced and the rise of temperature was noted at short intervals of time until the reaction was complete. The movement of the thermometer during repeated trials, in which all conditions remained the same, excepting that in alternate experiments the electro-magnet was active and inactive, served to bring out its influence upon the speed and character of the reaction and upon the amount of heat produced.

The conditions were varied as to initial temperature, nature and strength of the acid used and relative amounts of iron and acid. In the following detailed account, the various determinations are described in the order in which they were made.

I. *Experiments with Aqua-regia and Iron.*

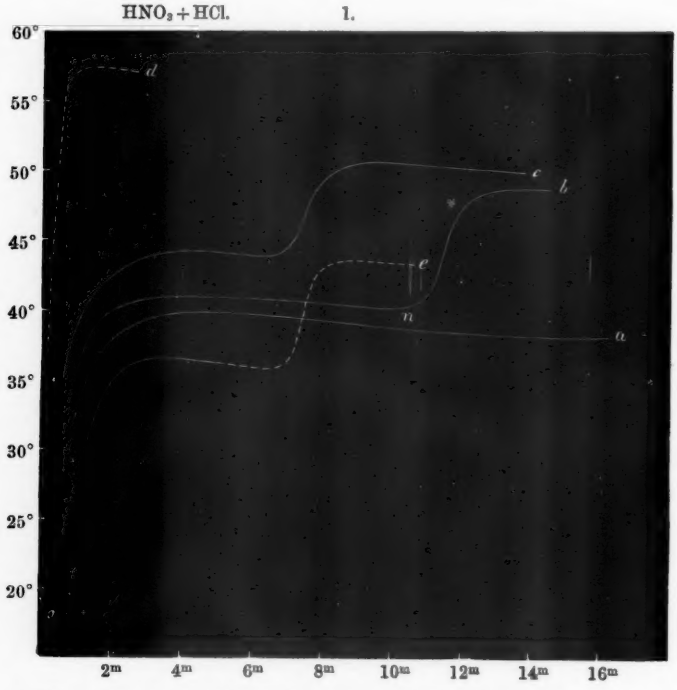
Nitric and hydrochloric acids were mixed with water in proportions which ensured rapid and complete solution of iron without the application of heat. A suitable combination was found by trial to consist of 4 vols. HCl, 3 vols. HNO_3 and 2 vols. H_2O. The acids were the usual concentrated "chemically pure" acids of commerce. 50^{cc} of this mixture were allowed to act upon two grams of powdered iron of the kind known as "iron by alcohol."

The electro-magnet, already described, was placed, poles uppermost, in a glass jar and surrounded with dry sand in order to prevent any heating effect from the coils. A beaker holding 250 c. c. was placed directly upon the poles of the magnet. A large bell jar, through the neck of which were set the thermometer and an open glass tube, was placed over the jar containing the magnet in such position that the bulb of the thermometer dipped in the acid within the beaker.

* A Preliminary Note on the action of Acids on Iron in the Magnetic field: Proceedings Am. Association for the Advancement of Science, 1884.

† This appears to be an almost unworked field. Professor Ira Remsen, in his interesting paper on the "Deposition of Copper on Iron in the Magnetic field" (American Chemical Journal, vol. iii, No. 3, 1881), has furnished the earliest published evidence of the influence of magnetism upon chemical action. More recently the same author has announced the resumption of his experiments upon this subject (Science, vol. i, No. 2, and American Chem. Journal, vol. vi, No. 6, 1885.)

The open tube was attached to a water vacuum-pump and after an elapse of five minutes the temperature of the acid was noted. The bell jar was then raised, two grams of iron were thrown into the acid and the bell jar replaced. The vacuum-pump removed all fumes as soon as formed. The temperature of the rapidly effervescing solution was noted at intervals of thirty seconds until the reaction was complete. The violent character of the reaction made all stirring of the solution unnecessary. In order to detect the influence of the magnet a series of these experiments were made in which, as already stated, all other conditions were maintained unaltered and the magnet was alternately active and inactive.



When iron is dissolved in aqua-regia the character of the reaction varies greatly with the strength and temperature of the acid. At low temperatures and in weak acid, hydrogen is given off and the solution at the completion of the reaction is greenish, containing for the most part ferrous chloride.

At higher temperatures the reaction is more violent, red nitrous fumes are evolved and the resulting solution is yellow

with ferric chloride. Professor Ordway* has described very similar variations in the reaction between nitric acid and iron.

What may be termed the critical temperature, i. e. that at which this change of reaction occurred, was found to be 40°. Reactions in which the solution did not reach that temperature were accompanied by the evolution of hydrogen only, while those in which this temperature was passed were of the double character described by Ordway, the production of hydrogen being followed by that of nitrous fumes. The difference between these reactions is best shown by curves. Curves *a*, *b* and *c*, fig. 1, show the range of temperature during three experiments in which the initial temperature was varied within narrow limits for the purpose of determining the critical temperature. The magnet was not in action during any of these experiments, but in other respects the process was that already mentioned. The curves show the temperature of the solution as a function of the time. Ordinates denote temperature in degrees centigrade, abscissæ time in minutes. The curves "*b*" and "*c*" have two maxima, whereas "*a*" shows no such peculiarity. The portion *o n*, of the curve *o b*, for example, indicates the rise of temperature produced by the solution of the iron under conditions in which hydrogen alone was generated. Whenever the solution reached the critical temperature (40°), a second reaction followed, in which red fumes were given off and a new rise of temperature occurred. The higher the temperature reached during the first reaction the more promptly the second one took place; as may be seen from the diagram. Curves "*b*" and "*c*" show the double reaction, "*a*" represents one in which the critical temperature was not reached and no nitrous fumes appeared.

The first reaction made in the magnetic field, under conditions which would otherwise have ensured the evolution of hydrogen only, was marked by an almost immediate and violent outburst of red fumes accompanied by a correspondingly greater rise of temperature. The reaction was repeated many times and always with the same result. The modification in the reaction produced by magnetic influence may be seen by comparing curve "*d*," which represents the rise of temperature when the reaction took place in the field with curve "*a*." The speed of the reaction is much greater within the magnetic field than without, and the amount of heat produced is greater. The evolution of nitrous fumes and the yellow color of the

* "Malleable iron in dilute nitric acid, sp. gr. 1·03 to 1·06, is attacked with elimination of hydrogen and the production of a ferrous salt or with elimination of nitrous dioxide and the production of a ferric salt or without any evolution of gas." "Sometimes hydrogen is produced at first, then nitrous dioxide, when both ferrous and ferric salts will be formed." Watts' Dict. (Supplement.) See also Ordway, this Journal, II, vol. xl, p. 316.

resulting solution indicated that in the magnetic field the chemical changes were not the same as those produced under other circumstances.

When the above-mentioned reaction was performed below the critical temperature, it was found that the mere actuation of the magnet, at any time before the last particle of iron was dissolved, was sufficient to modify the reaction. The evolution of nitrous fumes always followed the closing of the circuit, in such cases, and the change in the character of the reaction was indicated by renewed rise of temperature. Curve "*e*" shows the range of the thermometer during one of these reactions. The change from the unbroken line to the dotted line shows the point at which the magnet was thrown into action.

Table I gives the observations from which curves *a*, *b*, *c*, *d*, and *e* are drawn. The reactions were repeated many times under the various conditions already described, but the variations from the typical results given in the table were not such as to render necessary the publication of all the observations.

TABLE I.—*Action of aqua-regia upon iron.*

TIME.	TEMPERATURE. Reaction "a" (magnet not acting).	Reaction "b" (magnet not acting).	Reaction "c" (magnet not acting).	Reaction "d" (magnet acting).	Reaction "e" (magnet not acting).	TIME.	TEMPERATURE. Reaction "a" (magnet not acting).	Reaction "b" (magnet not acting).	Reaction "c" (magnet not acting).	Reaction "d" (magnet acting).	Reaction "e" (magnet not acting).
0m 0s	18°90	20°20	24°70	24°38	19°80	8m 30s	39°30	40°40	50°40	----	43°80
0 30	27·30	28·10	31·10	----	----	9 0	39·20	40·30	50·40	----	43·70
1 0	32·40	33·50	37·80	57·45	34·35	9 30	39·10	40·15	50·30	----	43·00
1 30	35·60	36·80	40·90	57·58	25·70	10 0	39·00	40·00	50·10	----	----
2 0	37·50	38·85	42·50	57·32	36·27	10 30	38·95	39·90*	50·00	----	----
2 30	38·45	39·60	43·20	57·00	36·55	11 0	38·85	40·40	49·80	----	----
3 0	39·10	40·30	43·70	56·75	36·70	11 30	38·75	44·10	----	----	----
3 30	39·45	40·70	43·85	56·37	36·80	12 0	38·62	47·00	----	----	----
4 0	39·57	40·85	44·00	----	36·80	12 30	38·50	47·35	----	----	----
4 30	39·60	40·97	44·05	----	36·80	13 0	38·25	47·50	----	----	----
5 0	39·60	41·00	44·05	----	36·70‡	13 30	38·20	47·37	----	----	----
5 30	39·60	40·97	43·95	----	36·80	14 0	38·10	47·30	----	----	----
6 0	39·60	40·90	43·90	----	36·70	14 30	38·00	47·15	----	----	----
6 30	39·55	40·89	43·60	----	36·57	15 0	37·90	----	----	----	----
7 0	39·50	40·78	43·85†	----	36·45	15 30	37·85	----	----	----	----
7 30	39·45	40·62	49·4	----	42·80	16 0	37·75	----	----	----	----
8 0	39·40	40·50	50·2	----	43·50						

* Red fumes appeared. † Red fumes appeared. ‡ Magnet set into action.

It will be seen that the speed of reaction is greater in the magnetic field than without, and that the heat of chemical union is much greater. The production of nitrous fumes under the influence of the magnet and the yellow color of the resulting solution showed that the reaction was modified in chemical character as well as in intensity.

This form of experiment, however, is ill adapted for comparison of the total amount of heat of chemical union within and without the magnetic field, owing to the very different speeds of reaction. A series of determinations made by my assistant, Mr. W. S. Franklin, in which the reaction took place in a calorimeter, and the number of thermal units produced was measured, showed that the development of heat was always considerably greater within the field of the magnet. The results of this calorimetric series is given in Table II.

TABLE II.—*Calorimetric determination of the heat of chemical union within the magnetic field.* (By W. S. Franklin.)

	Magnet not acting.	Magnet acting.
Mean rise of temperature,	8·86°	11·03°
Calories per gram of iron dissolved,	1035·0	1288·8

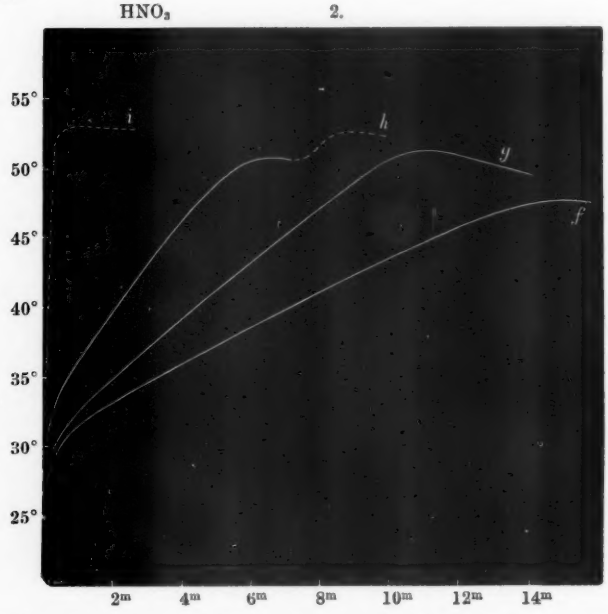
Water value of calorimeter and contents,	583·98 C.
Amount of iron dissolved in each reaction,	5 grams.
Duration of the reaction,	from 5^{mm} to 6^{mm}.

The reaction during these calorimetric determinations was not open to inspection as in the simpler form of experiment in which the variations of temperature alone were noted, but the range of temperatures within the calorimeter was such as to indicate that the reaction within the field was accompanied by the evolution of nitrous fumes, and outside the field by hydrogen only. Whether the amount of heat produced by a reaction in which nitrous fumes were given off outside the field is greater or less than in the corresponding form of reaction under the influence of the magnet was not determined.

II.—*Experiments with Nitric Acid and Iron.*

Nitric acid was mixed in various proportions with water for the purpose of finding the degree of dilution at which the most rapid, uniform and complete solution of the powdered iron could be obtained. During the search for the most desirable mixture, a new and very interesting effect of the magnet was accidentally discovered. Five grams of powdered iron lay in the beaker close above the poles of the electro magnet, which was in circuit. Some cold nitric acid was poured upon the iron but the latter remained passive. Wishing to note the character of the reaction, we warmed the beaker slightly, then placed it upon the poles of the magnet and put a thermometer into the solution to get its temperature. The bulb of the thermometer touched the iron in process of stirring the acid, when the hitherto passive mixture burst, almost explosively, into

effervescence, and a cloud of red nitrous fumes was given off. The removal of the solution from the field of the magnet restored the passivity of the iron, and the action, in a few seconds, ceased entirely. All attempts to destroy the passivity by further stirring failed, until the beaker was brought back into the neighborhood of the magnet, when, at the merest touch with the thermometer, or any glass rod, the reaction would begin again and continue until the beaker was removed from the field. It was found possible to render the iron passive and to destroy that passivity at will by the above process; and after the solution had become warm the reaction was found to begin spontaneously whenever the beaker was brought into the field.



The temperature at which the reaction begins spontaneously in the field was 60°, a point which probably varies with the strength of the acid and the character of the iron.

The further study of this curious phenomenon, which seems to be akin to the influence of the voltaic current upon the passivity of iron, described by Schönn* and by Renard,† was laid aside until a more powerful magnet could be procured.

*L. Schönn: Poggendorff's Annalen, Ergänzungs Bd. v, No. 2.
†A. Renard: Comptes Rendus, t. 79, Nos. 3, 5 and 7.

The proportions selected for the reaction of nitric acid on iron were 70 c.c. HNO_3 and 30 c.c. H_2O acting on two grams of iron. The speed of the reaction without the field was found to vary in a marked manner with the initial temperature. Brisk effervescence of hydrogen occurred, neither accompanied nor followed by the production of nitrous fumes. The effect of the magnet was to greatly increase the speed, reducing the average time from about eight minutes to less than one minute. Red fumes always resulted from the reaction within the field.

To change the character of the ordinary reaction, at any moment, it was only necessary, as in the case of the reaction with aqua-regia, to bring the magnet into function. The production of nitrous fumes, as in the former case was always accompanied by a new rise of temperature, although to a smaller extent than in the aqua-regia reaction.

Table III and curves "f" "g" "h" and "i" figure 2 show the range of temperature during the reactions with nitric acid. The variations of speed caused by slight changes in initial temperature were very marked. A comparison of the total amounts of heat produced within. and outside the field cannot be deduced from these curves because of the very different speeds of reaction.

TABLE III.—*Action of nitric acid upon iron.*

TIME.	TEMPERATURES.				TIME.	TEMPERATURES.			
	Magnet not acting.	Magnet not acting.	Magnet not acting.	Magnet acting.		Magnet not acting.	Magnet not acting.	Magnet not acting.	Magnet acting.
	"*f*"	"*g*"	"*h*"	"*i*"		"*f*"	"*g*"	"*h*"	"*i*"
	°	°	°	°		°	°	°	°
0m 00s	25·85	26·50	27·25	24·90	7m 30s	39·45	44·95	49·45*	----
0 30	29·65	29·90	34·75	52·50	8 00	40 18	46·10	49·37†	----
1 00	30·50	31·60	35·90	52·80	8 30	40·90	47·00	51·00	----
1 30	31·05	32·55	37·20	52·50	9 00	41·70	48·00	51·18‡	----
2 00	31·92	33·00	38·25	----	9 30	42·58	48·95	50·90	----
2 30	32·55	33·80	39·45	----	10 00	43·35	49·90	----	----
3 00	33·19	35·00	40·70	----	10 30	44·30	50·45	----	----
3 30	33·92	36·10	42·10	----	11 00	45·00	59·75	----	----
4 00	34·58	37·50	44·35	----	11 30	45·65	51·00	----	----
4 30	35·30	38·70	46·72	----	12 00	46·18	50·95	----	----
5 00	36·05	40·10	47·72	----	12 30	46·55	----	----	----
5 30	36·68	40·90	48·45	----	13 00	46·75	----	----	----
6 00	37·38	41·90	48 97	----	13 30	45·95	----	----	----
6 30	38·15	42·90	49·22	----	14 00	46·95	----	----	----
7 00	38·80	43·90	49·40	----					

* First maximum. † Magnet thrown into action. ‡ Second maximum.

III—*Experiments with Hydrochloric Acid and Iron.*

70 c.c. of the concentrated acid diluted with 30 c.c. of water were allowed to act upon two grams of iron. The rise of temperature was much smaller than in the determinations with aqua

regia and nitric acid, the speed of reaction within the magnetic field differed very little from that occurring under ordinary circumstances and the character of the reaction in the two cases was almost identical. The rise of temperature under the influence of the magnet, however, was found to be slightly in excess of that produced when the magnet was not in action. In order to bring out the magnetic influence more clearly than could be done with the magnet described in the first paragraph of this paper a larger one was constructed. The core of the new magnet was 25^{mm} in diameter, and each arm was wound with 840 turns of No. 18 copper wire. This magnet afforded a field many times as strong as that of the small one. With it the reactions of hydrochloric acid upon iron were repeated and all the remaining experiments of the investigation were made.

Considerable difficulty was found in obtaining concordant results with hydrochloric acid, owing to the frequently incomplete solution of the iron. The desired rapidity and completeness of action were however finally secured by using 50 c.c. of the undiluted acid upon two grams of iron at an initial temperature of 40°. Measurements under these conditions and with the larger magnet corroborated the results of the first series, and were free from the irregularities which had vitiated those determinations.

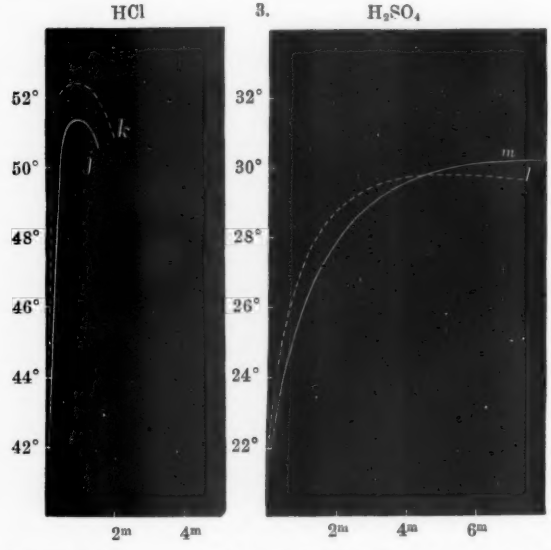


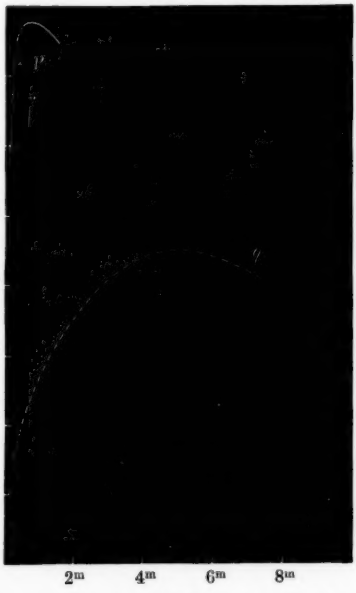
Table IV and curves "j" and "k," figure 3, show the range of temperature during these reactions.

TABLE IV.—*Action of hydrochloric acid upon iron.*

MAGNET NOT ACTING.		MAGNET ACTING.	
TIMES.	Temperatures. Reaction "*j*."*	TIMES.	Temperatures. Reaction "*k*."*
0m 00s	40°00	0m 00s	40°00
0 30	50·29	0 30	52·24
1 00	51·39	1 00	52·36
1 30	50·42	1 30	51·98
2 00	----	2 00	----

* Each column gives the mean of ten experiments.

The evidently greater amount of heat produced by the reaction within the field, might be attributed, it was thought, to the formation of a larger percentage of ferric chloride under the influence of the magnet. Volumetric determinations of the amount of ferric salt in the solution, by the hyposulphite of soda method, showed, however, that while the percentage varied between 2 per cent and 14 per cent, it was not larger when the reaction took place within the field than under ordinary circumstances. The rise of temperature was found, moreover, to be independent of the amount of ferric chloride present in the solution and its presence was thought to be due to oxidation after the completion of the reaction.



The reaction with hydrochloric acid was afterwards repeated in the presence of an excess of potassium chlorate, by which means all the iron was converted into ferric chlorate. The proportions used were 25cc HCl+25cc H_2O+1·5 grams $KClO_3$ acting upon 1 gram Fe. The reaction was rapid and complete, the solution was of a bright yellow color and free from all traces of ferrous chloride. In this case the amount of heat produced in the magnetic field was *much less* than when the magnet

remained inactive, and the speed of the reaction was less. Table V shows the character of these reactions and they are exhibited graphically in curves "p" and "q" (figure 4).

An attempt was made to eliminate the effect of cooling during these reactions by studying the rate of cooling of the solution and correcting the observed rise of temperature by means of data thus obtained. The corrected values are thought to indicate with considerable accuracy the relative amounts of heat produced in the two reactions. They are as follows:

$$\begin{cases} p \text{ (without mag.)}; & \text{total rise of temp.} = 33°{\cdot}85 \text{ corr. } 34°{\cdot}46 \\ q \text{ (with } \quad \text{)}; & \text{“ } \quad \text{“ } \quad = 17°{\cdot}85 \text{ “ } 24°{\cdot}48 \end{cases}$$

TABLE V.—*Solution of hydrochloric acid in presence of potassium chlorate.*

MAGNET NOT ACTING.		MAGNET ACTING.		MAGNET ACTING.	
TIMES.	Temp. Reaction "p"	TIMES.	Temp. Reaction "q"	TIMES.	Temp. Reaction "q"
0m 00s	40°·00	0m 00s	37°·90	3m 00s	55°·00
0 47	73·85*	0 30	45·90	3 30	55·70
----	----	1 00	48·65	4 00	56·70
----	----	1 30	49·95	4 30	57·85
----	----	2 00	52·20	5 00	57·10
----	----	2 30	53·55	5 30	----

* Owing to the great rapidity of the reaction, the only reading taken was at the maximum.

The reaction in which 3 grams of potassium chlorate were used presented no peculiarities such as to necessitate their presentation in tabular form. They were in all respects corroborative of the results given in table V.

IV.—*Experiments with Sulphuric Acid and Iron.*

The method was that already used with other acids.

25cc of the concentrated acid were mixed with 75cc of water, and the mixture when cool was made to dissolve 2 grams of iron. The reaction was uniform and complete and apparently of the same chemical character within and outside of the field. The magnet was found, however, to increase the speed of reaction and to decrease the amount of heat produced. The results are given in table VI, and in curves "m" and "l," figure 3.

TABLE VI.—*Action of sulphuric acid upon iron.*

MAGNET NOT ACTING.		MAGNET ACTING.	MAGNET NOT ACTING.		MAGNET ACTING.
TIMES.	Temp. "m"	Temp. "e"	TIMES.	Temp. "m"	Temp. "e"
$0^m\ 00^s$	21°·50	21°·50	$4^m\ 00^s$	30°·12	29°·84
0 30	23·60	24·25	4 30	30·18	29·86
1 00	26·18	27·15	5 00	30·20	29·83
1 30	27·92	28·60	5 30	30·20	----
2 00	28·95	29·61	6 00	30·17	----
2 30	29·45	29·75	6 30	----	----
3 00	29·82	29·80	7 00	----	----
3 30	30·00	29·85			

V.—*Experiments with Nitric Acid and Copper.*

In conclusion of this investigation a series of measurements were made by the methods already described, in which powdered copper, such as is obtained by electrolytic deposition, was substituted for iron. The character of the reaction within and outside of the magnetic field was noted and especial attention was paid to the matter of speed and heat of chemical union. The reaction in the field was found to be identical in both respects with that which occurred when the magnet was not in action. The mean of time determinations showed, for instance, the rise of temperature and duration of action to be:

Within the magnetic field.		*Outside of the magnetic field.*	
Rise of temperature	$=4°·875$	Rise of temperature	$=4°·882$
Duration	$=4^m04·5^s$	Duration	$=4^m05·6^s$

In these experiments 20^{cc} of nitric acid and 30^{cc} of water acted upon 1 gram of copper. Various cursory tests with other metals seemed to indicate that the effects of the magnet described in this paper are confined to reactions in which iron is one of the factors.

The set of experiments described in the preceding paragraphs is to be regarded as preliminary to a more complete investigation of the novel series of effects developed by them. It is my intention to repeat these determinations with a large electromagnet now in process of construction. The further study of the destruction of the passivity of iron by magnetic action is indeed already well advanced and the results will soon be published.

University of Kansas,
Lawrence, Kansas, August, 1885.

ART. XXVII.—*The Inculcation of Scientific Method by Example, with an illustration drawn from the Quaternary Geology of Utah;* by G. K. GILBERT. (With a map, Plate VIII.)

Presidential Address read before the American Society of Naturalists at Boston, December 27, 1885.

Mr. President and Gentlemen:—This is an association of teachers of science and investigators. Those of us who are primarily engaged in investigation have come here more especially as educators. It is our function to discuss, not our results, nor the subject matter of the several sciences with which we are concerned, but our methods of investigation, our methods of publication or promulgation, our methods of teaching.

It is fitting that this, one of the first formal addresses before the Society, should deal with some of the most general considerations affecting methods. In the statement of these considerations it is impossible to avoid that which is familiar, and even much that is trite. Indeed all expectation of entertaining or edifying you with the original or the new may as well be disclaimed at the outset. I shall merely attempt to outline certain familiar principles, the common property of scientific men, with such accentuations of light and shade as belong to my individual point of view.

The teacher's work is susceptible of a logical division into two parts. He stores minds, and he trains them. The modern educator believes the second function to be the higher, because the trained mind can store itself. Nevertheless the two go hand in hand and are in great part inseparable. The effort of the intelligent teacher is to employ such methods in storing the minds of his pupils with knowledge that they shall acquire at the same time the best training.

In that particular department of teaching which is called scientific, there is the same logical duality, and to a great extent there is a practical unity; but in this case there is a pre-determined classification of those who fall under the teacher's instruction, which has the effect of practically dividing his methods. A portion of his pupils are preparing to engage in the work of research, and look to a scientific career. Another portion are to be occupied with business or in other pursuits not implying research, at least in the ordinary sense, and desire to obtain, as a part of a liberal education, an acquaintance with the materials and results of science. The first demand a training in methods, the second consciously ask only for a store of knowledge. Nevertheless, the general student can best accomplish his purpose with the aid of a certain

amount of training in method, while to him who proposes a career of investigation, there is an equal necessity for a large amount of positive knowledge.

Before proceeding to amplify these propositions it seems best to give consideration to the essential nature of scientific research—to restate, for the sake of a common understanding, the process by which science advances.

Scientific research consists of the observation of phenomena and the discovery of their relations. Scientific observation is not sharply distinguished from other observation. It may even be doubted whether there is such a thing as unscientific observation. If there is a valid distinction, it probably rests on the two following characters. Scientific observation, or the observation of the investigator, endeavors to discriminate the phenomena observed from the observer's inference in regard to them, and to record the phenomena pure and simple. I say "endeavors," for in my judgment he does not ordinarily succeed. His failure is primarily due to subjective conditions; perception and inference are so intimately associated that a body of inferences has become incorporated in the constitution of the mind. And the record of an untainted fact is obstructed not only directly by the constitution of the mind, but indirectly through the constitution of language, the creature and imitator of the mind. But while the investigator does not succeed in his effort to obtain pure facts, his effort creates a tendency, and that tendency gives scientific observation and its record a distinctive character.

Scientific observation is moreover selective and concentrated. It does not gather facts indiscriminately, but, recognizing their classification, it seeks new facts that will augment established groups. The investigator, by restricting his observation to a limited number of groups of phenomena, is enabled to concentrate his attention, and thus sharpens his vision for the detection of matters that are unnoticed by the ordinary observer.

The superficial relations of phenomena are discovered by induction—by the grouping of facts in accordance with their conspicuous common characters—or, in other words, by empiric classification. Such empiric classification is a preliminary work in all sciences. It is a convenient and temporary sorting of our knowledge, and with the increase of knowledge it is perpetually remodeled. But it is more than a mere convenience; it is a stepping-stone to a logical, or rational, or, more strictly, relational classification; for it leads to the understanding of those deeper relations which constitute the order of nature.

Phenomena are arranged in chains of necessary sequence. In such a chain each link is the necessary consequent of that

which precedes, and the necessary antecedent of that which follows. The rising of the sun is consequent on the rotation of the earth. It is the logical antecedent of morning light. Morning light is in turn the consequent of sunrise and the antecedent of numerous other phenomena. If we examine any link of the chain, we find that it has more than one antecedent and more than one consequent. The rising of the sun depends on the position of the earth's axis as well as on its rotation, and it causes morning heat as well as morning light. Antecedent and consequent relations are therefore not merely linear, but constitute a plexus; and this plexus pervades nature.

Relational classification may be considered as of two sorts, first linear, and second, coördinate as determined by linear, that is to say, phenomena are linearly arranged in chains of sequence, and they are coördinately arranged in natural classes. A natural class is a group of coördinate facts having the same antecedents.

It is the province of research to discover the antecedents of phenomena. This is done by the aid of hypothesis. A phenomenon having been observed, or a group of phenomena having been established by empiric classification, the investigator invents an hypothesis in explanation. He then devises and applies a test of the validity of the hypothesis. If it does not stand the test he discards it and invents a new one. If it survives the test, he proceeds at once to devise a second test. And he thus continues until he finds an hypothesis that remains unscathed after all the tests his imagination can suggest.

This, however, is not his universal course, for he is not restricted to the employment of one hypothesis at a time. There is indeed an advantage in entertaining several at once, for then it is possible to discover their mutual antagonisms and inconsistencies, and to devise crucial tests,—tests which will necessarily debar some of the hypotheses from further consideration. The process of testing is then a process of elimination, at least until all but one of the hypotheses have been disproved.

In the testing of hypotheses lies the prime difference between the investigator and the theorist. The one seeks diligently for the facts which may overthrow his tentative theory, the other closes his eyes to these and searches only for those which will sustain it.

Evidently, if the investigator is to succeed in the discovery of veritable explanations of phenomena, he must be fertile in the invention of hypotheses and ingenious in the application of tests. The practical questions for the teacher are, whether it is possible by training to improve the guessing faculty, and if so, how it is to be done. To answer these, we must give attention to the nature of the scientific guess considered as a mental

process. Like other mental processes, the framing of hypotheses is usually unconscious, but by attention it can be brought into consciousness and analyzed.

Given a phenomenon, A, whose antecedent we seek. First we ransack the memory for some different phenomenon, B, which has one or more features in common with A, and whose antecedent we know. Then we pass by analogy from the antecedent of B, to the hypothetical antecedent of A, solving the analogic proportion—as B is to A, so is the antecedent of B to the antecedent of A.

Having thus obtained an hypothesis, we proceed to test it. If the hypothetical antecedent is a familiar phenomenon, we compare its known or deduced consequents with A, and observe whether they agree or differ. If it is unfamiliar, we ascertain its consequents by experiment or some other form of observation; and in the selection of the particular experiments or observations to serve as tests, we are guided once more by analogy, inverting the previous formula.

The question, whether or not the function of the mind in devising hypotheses and the tests of them is creative, is foreign to the present purpose. It suffices that we recognize the process as analogic, requiring for its success a preliminary knowledge of numerous instances of consequential relations. The consequential relations of nature are infinite in variety, and he who is acquainted with the largest number has the broadest base for the analogic suggestion of hypotheses. It is true that a store of scientific knowledge cannot take the place of mental strength and training, i. e. of functional ability inherited and acquired, but it is nevertheless a pre-requisite of fertility in hypothesis.

The great investigator is primarily and preëminently the man who is rich in hypotheses. In the plenitude of his wealth he can spare the weaklings without regret; and having many from which to select, his mind maintains a judicial attitude. The man who can produce but one, cherishes and champions that one as his own, and is blind to its faults. With such men, the testing of alternative hypotheses is accomplished only through controversy. Crucial observations are warped by prejudice, and the triumph of the truth is delayed.

Returning now to the subject of education, take first the case in which the student is to become an investigator. He is to observe phenomena, he is to frame and test hypotheses. As a matter of course, in order to learn to do these things he must do them. Sooner or later he must be sent directly to nature, out of doors or in the laboratory, and must in her presence train his faculties by practice. But before he undertakes this, the teacher can aid him by imparting methods. It is probably

not best to offer them in the abstract until he has become well acquainted with them in the concrete. Typical investigations should be described in detail, illustrating the varied phases of the method of hypothesis, and not omitting to show how its successes are achieved through series of failures. The history of at least one science should be developed, with the rise and fall of its successive theories. These educational factors are directed to the training of his mind, but his mind needs also to be stored with scientific knowledge, which shall serve as a foundation for analogies. If he would explain some feature of nature, he must depend on the explanations others have reached for other features; and he needs large resources of knowledge of the relations of phenomena.

The course of training for the apprentice of science should give him, in the study room and in the class room, a varied acquaintance with the laws of nature that have been discovered by research. It should not needlessly burden his memory with empiric classifications, for these belong to the humbler walks of science, and it is unwise to impress on the novice the high importance of that which the master regards as provisional. It should teach observation by actual practice,—practice rigorously restricted to selected groups of phenomena. It should illustrate with varied reiteration—by books, by lectures, by demonstrations in the laboratory—the method of discovery by the aid of hypotheses. It should assign him actual investigation and subject his methods to criticism.

Students whose projected careers are not scientific, but who are unwilling to ignore so important a subject, naturally wish to cover a wide field in a short time. Their teacher, imbued with the vastness of science, is tempted to give them a maximum number of facts, with such order and classification as best favor their rapid statement. If he yields to the temptation, there is reason to fear that a permanent misapprehension is established, and the essence of science is not communicated. In my judgment he will do better to contract the phenomenal, and enlarge the logical scope of his subject, so as to dwell on the philosophy of the science rather than its material. For such students laboratory work may or may not be expedient, but they can at least accompany the pupils who look forward to careers in research in some of the descriptive illustrations of methods of scientific achievement.

The investigator becomes an educator when in giving his work to the world he describes the route by which his end was reached. It is not denied that the publication of sound conclusions is in itself educational, but it is maintained that the publication of the concrete illustration of a good method is educational in a higher sense. It is not insisted that all skillful

investigations should be published *in extenso;* it is only affirmed that the number of such publications is far too small. We need for educational purposes more narratives of good work in all departments of research. Let the discoverer of a new principle recite every hypothesis that occurred to him in the course of his search, telling, if he can, how it was suggested. Let him lay bare the considerations which rendered it plausible, the tests that were conceived, and those which were applied. Let him show in what way the failure of one hypothesis aided in the invention of another. Let him set forth not only the tests which verify his final hypothesis, but the considerations which leave a residuum of doubt as to its validity. And finally let him indicate, if he can, the line or lines of research that promise to throw more light.

By so doing he will accomplish many things. He will guard himself against an overestimate of the strength of his uneliminated hypothesis, and he will thus diminish his self-conceit. By conscious attention to his methods he will improve them. He will therefore educate himself.

He will inspire the young investigator by his example, and even his experienced compeer will take courage from the success that after many failures finally crowns his efforts. He will give to every investigator who reads his paper a lesson in method—a good lesson if his method is good, and not necessarily a bad one if his method is bad. He will therefore educate his fellow workers.

If his work admit of popular presentation, he may be a missionary as well as a teacher, for he may help to dissipate the wide-spread impression that there is something occult in the ways of science. He will at least aid in showing that, whenever a theory is created and tested, knowledge is the gainer, whether the theory itself stands or falls; and that the demolition of hypotheses, instead of testifying to the futility of research, is the method and condition of progress. His educational influence will thus extend to that great lay member, the general reader.

In making this plea for education by example, it would be unfair to ignore another point of view. Not all are willing to be educated, not all need be; the majority of those who examine an essay seek only to learn its conclusions and have time for nothing more. For their use there should be appended or prefaced a concise summary of results.

And, on the other hand, it should be observed that the service rendered to science by one who describes his course of investigation is not educational merely. Rejected hypotheses have a positive value in the domain of the subject to which they belong, and he who makes them public gives to his

fellow-workers in the special field the fullest advantage of his material. Some steps of his progress, which did not prove suggestive to him, will find fertile ground in the mind of another and bear fruit. This consideration places the progress of knowledge before the glory of the individual, and is opposed by a natural egoism; but it is only the man of small calibre who has no ideas to spare, and secretiveness in matters of science is ordinarily a confession of weakness.

It was intimated a moment ago that precept unsupported by example could not be depended on to infuse method—and the dictum applies even to the burden of this discourse. I am persuaded that my meaning will be better apprehended if I supplement my disquisition by an outline of an investigation of my own. The seeming egotism must be condoned, for it is manifestly impossible for me to trace out the actual course of observation and reasoning in the case of another's work.

To guard against possible misapprehension it is necessary to emphasize the fact that the following discussion contains an outline merely of its subject. Its subject is a certain geologic uplift that has been observed in Utah. To render it intelligible to those who are unacquainted with the literature of the geology of Utah, it will be introduced by a short account of Lake Bonneville.

The basin of Great Salt Lake lies in a region of mountains; to picture its character to your mind, conceive a plain the surface of which is embossed by parallel ridges of moderate length, from fifteen to twenty-five miles apart, and from 2,000 to 6,000 or 7,000 feet high. Conceive further that portions of this plain are uplifted, together with their mountain ridges, so as to enclose a basin 150 miles in either dimension, and you have the general structure of the district in question. The debris washed down from the mountains has for ages accumulated in this depression, so that the central-lying mountain ridges are nearly buried; indeed there is reason to suspect that some of them are quite buried, a plain of fine silt being spread smoothly over them. Great Salt Lake itself lies on the east side of the basin; the western half, which is only a few feet higher, is a saline desert.

In the last geologic epoch—the Glacial Epoch—the lake expanded so as to fill the basin to overflowing. The water surface was then very much larger, and as its area included the basins of several lakes now independent, it has been given a separate name. Lake Bonneville was very irregular in form; the mountain ranges of the basin ran long peninsulas from its north and south shores, and projected from its surface in numerous islands. The Quaternary winds, playing on its surface, dashed waves against its shores, and the spits and beaches and

cliffs wrought by these waves remain in a high state of preservation to testify to the position of the ancient water margin. Through the greater part of its extent this shore-line forms a conspicuous feature in the topography of the country, and is readily traceable. It has been actually traced out, surveyed, and mapped with much care, and our knowledge of the old lake is in many other respects definite and full.

Its width from a few miles east of Great Salt Lake to the west side of the Great Salt Lake desert was 125 miles. From the mountains on the north of the desert to those on the south its expanse was about the same, but it did not terminate with the southerly mountains. It extended through them in several straits and formed beyond a second and much smaller body of water. The main body was 1,000 feet deep, the minor body about 500 feet.

Manifestly, when this old shore-line was made, all parts of it lay in the same horizontal plane, with no other curvature than that which belongs to the figure of the earth; that is, all parts of it were level. If it is not now level—if some parts are higher than others, it seems equally manifest that there have been local elevations or subsidences of the land. It is to such differences of level in the shore-line as it stands, and to their interpretation, that I desire to call your attention.

As far as the eye can judge, the shore is still level, and so long as no measurements were made its horizontality remained unquestioned. The two geologists who were probably the first to measure its height recorded their results in language implying no suspicion that more than one determination was necessary.

It happened that in the year of my first exploration of the Bonneville area I saw the shore-line not only in the Salt Lake basin but in the more southerly basin, and that I passed from one basin to the other by a route that did not reveal their connection. In doubt whether two lakes were under observation or only one, I sought to answer the question by determining the height of the shore-line in each, my instrument for the purpose being the barometer. The verdict of the barometer was that the southerly shore-line was somewhat higher than the northerly, but the computations necessary to deduce it were not made until the mutual continuity of the two shore-lines had been ascertained by direct observation. The barometric measurement was therefore superseded as an answer to the original question, but it answered another which had not been asked, for it indicated that the ancient shore at one point had come to stand higher than at another. The postulate of horizontality was thus overthrown.

An hypothesis immediately took its place. It is one of the

great inductions of geology that as the ages roll by the surface of the earth rises and falls in a way that may be called undulatory. I do not now refer to the anticlinal and synclinal flexures of strata, so conspicuous in some mountainous regions, but to broader and far gentler flexures which are inconstant in position from period to period. By such undulations the Tertiary lake basins of the Far West were not only formed but were remodeled and rearranged many times. By such undulations the basin of Great Salt Lake was created. As to their cause, geology is absolutely ignorant, and she is almost absolutely silent. When it was ascertained that the Bonneville shore-line attwodistant points had not the same height, the first hypothesis to suggest itself merely referred the difference to this gentle undulatory movement of the crust. As other hypotheses are to be mentioned, it will be convenient to christen this one the hypothesis of unexplained undulation.

A few years later the discovery was made that a fault had occurred along the western base of the Wasatch range of mountains since the Bonneville epoch. This range lies just east of Great Salt Lake, and the Bonneville shore is traced across its western face. The effect of the fault was to lift the mountain higher, with reference to the lake bottom, and to carry that part of the old shore-line upward. The amount of the uplift varied in different parts of the fault from ten to fifty feet.

This discovery was something more than the finding of a post-Bonneville fault; it was the discovery also of a new method of recognizing faults—of a peculiar type of cliff produced by faulting, which, though by no means obscure, had previously been overlooked by geologists. It gave rise to a new tentative explanation for the displacement of shore-line discovered by barometer, namely, that it arose by faulting; and it opened a new line of observation.

Two hypotheses were now under consideration, but they were not strictly alternative. Perhaps it would be better to say that the origination of the second not only gave an alternative but also modified the first. The Wasatch fault must affect the height of the shore-line, and wherever it crossed the shore-line that line must be discontinuous and exhibit two levels. The admission of disturbance by faulting was therefore compulsory, but faulting might or might not be sufficient alone. If it was not sufficient, then undulation might complement it. The modified first hypothesis was, undulation and faulting combined, the second, faulting alone—both undulation and faulting being themselves unexplained.

It was not difficult to devise tests. An instrumental level line might be carried along the old beach so as to ascertain whether it rose or fell in regions where no faults occur; or its

height might be carefully measured at two points and the difference of altitude compared with the total throw of the intervening post-Bonneville faults. The second of these tests was applied, and with success. By means of the surveyor's level the height of the old shore above the water surface at the shore of Great Salt Lake was measured at two points twenty miles apart. One of these points is on the Wasatch range near Salt Lake City; the other is on the next range west, the Oquirrh. The only post-Bonneville fault between them is that at the base of the Wasatch, and its throw is there about fifty feet, the west side having gone down. If then faulting is alone responsible for shore-displacement, the beach on the Oquirrh range should be fifty feet lower than that on the Wasatch. The measurement however showed it to be twenty-eight feet higher, and thus demonstrated a difference of seventy-eight feet to be referred to undulation.

Thus a step was made in advance, but the resulting position was not final, for the inquiring mind could find no satisfaction in the knowledge that crust undulation and crust faulting were conjointly efficient, so long as both these remained without explanation. No new hypotheses were at once invented, but it was determined to continue observation until the solitary phenomena at command were expanded into a group, and to seek new light in the classification of this group. As the basin was traversed in the conduct of the general investigation of the old lake, a search was made for the records of recent faults and at every opportunity the height of the shore was accurately measured. Six such measurements were made in the immediate vicinity of Great Salt Lake, the lake affording a common datum plane. Ten others were made on the lines of railways, where the leveling data of the railway engineers could be utilized. At some points the height was found greater than on the Oquirrh, at others less than on the Wasatch, the range from highest to lowest being 168 feet.

Faults were discovered at the bases of numerous mountain ranges, but none of them are so great as that along the Wasatch, and nearly all are very small. None were found associated with the half-buried mountains of the center of the desert, and yet on these same mountains are the highest shore records to which measurement was carried.

In general it was found that the displacements recorded by the shores have been much larger than the displacements demonstrated by faults, so that faulting can be appealed to in explanation of shore displacement only to a small extent. It was found that the throw of the faults was in some cases opposed in direction to the total deformation on the shore-line and in other cases coincident. For these reasons faulting was provis-

ionally regarded as a disturbing factor merely, and the deformation demonstrated by the measurements of shore-height was treated as simply flexural or undulatory.

To classify the shore-heights, as a basis for further hypothesis, they were platted on a map, and their grouping was compared with geographic features. It appeared that the highest measured points lay within the area of the main body of Lake Bonneville, that the lowest points lay at the extreme north and at the extreme south, that the eastern shore of Lake Bonneville in the vicinity of Great Salt Lake was intermediate in height, and that the single point determined on the western shore of the old lake agreed in height with the eastern shore. Unfortunately, the distribution of the measurements, which had been largely determined by the distribution of railroad lines, was not equable throughout the basin of the lake, and nearly the whole of its western shore was undetermined in altitude. When lines of equal altitude were drawn among the figures representing measurements, after the manner of the isobars on a Signal Service weather map, it was found that they were not fully controlled by the determined points; but when they had been given the most satisfactory adjustment, they contoured a figure of deformation which may be characterized as a low, broad dome, having its crest over the center of the main body of Lake Bonneville, and extending a subordinate member to the region of the southern body of the lake. One half of this figure was fairly inferred from the data of observation; the remaining half was imaginary and its drawing merely gave graphic expression to the hypothesis suggested by the incomplete contours—the hypothesis that the deformation stands in some necessary or causal relation to the lake and its disappearance.

Now it has been independently determined that the cause of the lake and the cause of its disappearance were climatic; it was not drained by the wearing down of its outlet, nor emptied by the unequal uplift of portions of its rim, but it was dissipated by evaporation. If then the disappearance of the lake and the deformation of the land are connected in a causal way the change in the lake was the cause, and the change in the land was the effect. How can we suppose the drying up of the water to have produced the up-arching of the plain on which the water lay?

In the attempt to answer this question three tentative explanations were suggested, and these will be stated in the order of their origination.

It is well known to geologists that in several instances a great formation thousands of feet in thickness consists wholly or chiefly of shore deposits. To account for them it is neces-

sary to suppose that the sea floor locally sank down as rapidly as the sediments were added. Conversely there is reason to believe that the adjacent continent, which by erosion furnished the sediment, rose up as rapidly as its surface was degraded. It is a favorite theory—at least with that large division of geologists who consider the interior of the earth as mobile—that the sea-bottom sinks in such cases because of the load of sediment that is added and that the land is forced up hydrostatically because it is unloaded by erosion. A similar theory might explain the up-arching of the desiccated bed of Lake Bonneville, for the unloading of 1000 feet of water from an area more than one hundred miles across would give to the supposed liquid interior an irresistible uplifting force. This was the first explanation to suggest itself.

The second suggestion did not spring from any geological theory consciously retained in memory, but I have since suspected that the germ of the idea may have been caught from a passage in Croll's 'Climate and Time.' It is this: The geoid of which the ocean's surface is a visible portion is not an ellipsoid of revolution, but differs from that symmetric surface by undulations which depend on local inequalities in the density and in the superficial configuration of the earth. The water level is everywhere normal to the plumb-line, but the plumb-line, as geodesy has shown, is subject to local deflection. Now the ocean itself is one of the attracting factors, and if the ocean were to be removed, the geoid would thereby be modified. The surface of Lake Bonneville was part of a geoid at a higher plane than that of the ocean surface, and the removal of the water of the lake unquestionably modified the local form of the geoid. Only at first blush the cause seems too small for the effect observed.

The third suggestion relates to the distribution of temperatures beneath the surface of the earth. It is well established that the inner parts of the earth are extremely hot. The outer surface is relatively cool, and in the intermediate region there is a gradation of temperatures. The isogeotherms, or planes of equal temperature, are not even surfaces, but undulate in response to variations of conductivity and of superficial temperature. At the poles, where the external surface of the crust is exceptionally cold, the isogeotherms lie lower down than in warmer latitudes; and if a portion of the earth's surface undergoes a permanent change in temperature, the influence of this change is propagated slowly downward through the crust, and the isogeotherms are locally raised or lowered. Where they are raised, the crust is locally expanded, and its surface is uplifted; where they are depressed the surface of the crust subsides. If, therefore, it can be shown that the temperature at the

bottom of Lake Bonneville was raised in connection with the dessication of the lake, we have a true cause of upward movement, and if we can show furthermore that the temperature of the surrounding region was not equally raised, we have at least a qualitative explanation of the differential uplift, the phenomenon to be accounted for.

It is now several years since these explanations were first suggested, and subsequent reflection has developed no others. While all of them appear perfectly rational, only a very slight inspection was necessary to raise a doubt as to the quantitative sufficiency of the second and third. It was therefore determined to ascertain as accurately as possible the maximum change which might be ascribed to each of the three suggested causes, and to compare it with the actual change. The actual change is susceptible of various statements. If we consider only the measurements on the margin of the main body of water, and in its center we find a difference of 100 feet; by including observations on outlying bays we get a maximum difference of 168 feet; and a study of the peripheral slopes of the uplift suggests that they extend somewhat beyond the boundaries of the lake. Crude extrapolation gives 200 feet as a maximum estimate of the height of the crustal dome.

Take first the hypothesis that the crust of the earth, floating on a molten nucleus, rose up in the region of the basin when its weight was locally diminished by the removal of the water of the lake. The weight of the load removed is measured by the depth of the water before evaporation, 1000 feet. The theory supposes that as the crust rose there flowed in beneath enough molten rock to replace the weight of the evaporated water. If the rock was very heavy, a layer of moderate depth was necessary; if it was less heavy, more was required; but in any event the thickness of the introduced layer must be equal to the amount of the superficial uplift. It is known that the density of the earth's material increases downward, for the mean density of the earth, expressed in terms of the density of water, is about 5·5, while that of the upper portion of the crust is about 2·7. Nothing is known however of the law under which the density increases, and nothing is known as to the depth of the zone at which matter is sufficiently mobile to be moved beneath the Bonneville basin. We may, however, indicate limits, and I think this is fairly done by assuming that the density of the introduced matter was not less than 3, nor more than 5·5. If it was 5·5, the uplift consequent on the evaporation of 1000 feet of water would be 182 feet. If it was 3, the uplift would be 333 feet. Now it has already been stated that the greatest value observation suggests for the amount of the uplift is 200 feet. The postulate is therefore abundantly competent in a quantitative way.

To evaluate the effect produced under the second hypothesis (the hypothesis, that is, that the geoid represented by the water surface of Lake Bonneville has been deformed by the withdrawal of the attraction exerted by the water itself) it is necessary to employ mathematical analysis of a high order. As my schooling in mathematics did not qualify me to undertake this, I submitted the problem to an eminently competent colleague, who has solved it rigorously and deduced for the deformation of the geoid within the area of Lake Bonneville a maximum amount of two feet. The second explanation is therefore eliminated from consideration, because quantitatively insufficient.

It remains to consider the rise of the isogeotherms, and to evaluate the resulting elevation of the basin. It has been established by numerous observations that in all lakes having a depth as great as 1000 feet, the temperature at the bottom is about 39° F. This depends upon the fact that water at that temperature is heavier than at any other, and having once reached the bottom of a deep lake, it is withdrawn from the circulation to which the upper layers are subject, and remains undisturbed. The meteorological records show that the mean annual temperature of the desiccated basin of Lake Bonneville at the present time is 52°. The change from a humid to an arid condition has therefore raised its temperature 13°. The temperature of the surrounding regions has at the same time undergone a change, of which we have no precise estimate. The epoch of Lake Bonneville was the Glacial Epoch, and the local climate was then in all probability cooler. If it was 13° cooler, the isogeotherms would be no more affected at the center of the basin than at its margins, and there would be no differential elevation. If it was cooler by less than 13° a differential uplift would occur. For the sake of giving this uplift a maximum value, we will assign a very small figure to the general change of temperature, namely 3°, and assume that the differential change with respect to the basin was 10°. A formula devised by Fourier enables us to estimate the rise of the isogeotherms, if only we know the conductivity of the material of the earth, and the time which has elapsed since the Bonneville shore line was carved. Then, if we know additionally the rate of expansion of rock for a degree of temperature, we are able to estimate the upheaval. Sir William Thompson has determined experimentally a coefficient of conductivity. The late Prof. Bartlett, of West Point, has determined the coefficient of expansion for several building stones, which may be assumed to represent the crust beneath the Bonneville basin. We do not know how these coefficients are affected by high temperatures and great pressures, such as exist deep in the crust, and an element of uncertainty attaches for that reason.

In order to obtain a maximum result despite this uncertainty, I have made an extreme assumption in regard to time. The shore line of Lake Bonneville is in a wonderfully perfect state of preservation. While one stands upon it, it is easy to believe that it is but a few centuries old, and the geologist, accustomed as he is to the contemplation of eons of time, hesitates to estimate its antiquity in greater units than thousands or at most tens of thousands of years. When therefore we postulate its antiquity at one hundred millions of years, we pass so far beyond the range of probability as to protect ourselves against a possible underestimate. With these data the computation has been made, and it has been ascertained that a maximum uplift of 36 feet* can thus be accounted for. Since observation shows an uplift of not less than 100 feet, the thermal explanation is shown to be entirely inadequate; and if we were able to substitute for our imperfect data the actual data, we should probably find the computed uplift too small to be taken into consideration.

If therefore we admit that the removal of the water of the lake was the cause of the upheaval of the lake-bottom, there seems no way to avoid the conclusion that the efficient *modus operandi* was an upbending of the solid crust of the earth, caused by hydrostatic pressure communicated through a mobile substratum. But we are far from being forced to that admission. The coincidence in locus of the uplifted dome and the Quaternary lake may have been fortuitous; or there may even have been no coincidence, for the contoured figure of deformation was in part supplied by the imagination; and in either of these cases we can fall back on the agnostic hypothesis of unexplained undulation. In the present state of observation and inference the hypothesis of the hydrostatic restoration of equilibrium by the underflow of heavy earth-matter is the only explanation which explains, and none of the observed facts antagonize it; but the alternative hypothesis is not barred out.

To reach a satisfactory conclusion more observation is necessary, and this discussion of the subject would be premature were it not that the necessary observation is very expensive, and there is no immediate prospect that it will be supplied. It is fitting, however, that the desirable lines of research be pointed out.

The undertaking that promises most is an exhaustive hypsometric survey of the Bonneville shore line, including all bays and islands. If this were executed, it would be possible to deduce

* In the original paper, as read, 12 feet instead of 36 were erroneously given. A friend has since pointed out that the estimate of 12 feet includes expansion in the vertical direction only, whereas the coincident horizontal expansion would almost necessarily be converted into uplift.

a much more satisfactory expression of the shape of the uplift, and to determine either that it is intimately related to the form of the body of water removed, or that it is not so related. If the relation were demonstrated, the observations might so far indicate its nature as to render possible an evaluation of the rigidity of the earth's crust.

Another profitable method of continuing the inquiry would be to make a similar investigation of the shore of another extinct lake, for example, the one to which Clarence King has given the name of Lahontan, and which ranks second to Lake Bonneville among the Quaternary lakes of the Great Basin. If in a second instance the center of the desiccated lake were found to be the locus of upheaval, the hydrostatic theory would be practically established.

It is hardly necessary for me to assure you that my personal regret in abandoning this research at its present stage is very great. I have discussed it as an investigation of the deformation of the Bonneville basin, but it has a broader meaning. The condition of the interior of the earth is one of the great problems of our generation. Those who have approached it from the geologic side have based a broad induction on the structural phenomena of the visible portion of the earth's crust, and have reached the conclusion that the nucleus is mobile. Those who have approached it from the physical and astronomic side have reached the conclusion that the nucleus is rigid. Here seems an opportunity for a crucial observation. If the crust of the earth floats upon a fluid nucleus, the evaporation of Lake Bonneville, by lifting from it a great weight, must have produced an uplift of determinate form. If the whole earth is solid, such a result could not have been wrought. The decisive phenomena are known to exist, and to be accessible, but they are scattered over a broad desert, and they can be gathered in only at the cost of much money and great labor.

ART. XXVIII.—*Nova Andromedæ;* by ASAPH HALL.

(Communicated by Geo. E. Belknap, Commodore U. S. N., Superintendent.)

AT the end of last August Dr. Hartwig, of Dorpat, announced the appearance of a new star in the great Nebula of Andromeda. This announcement aroused much interest and brought out a great deal of speculation on the origin of these new stars, and on the nature of the nebulæ and their connection with our sidereal system. Quite naturally in such a case,

much of this speculation was of the wildest character, and some very uncertain inferences were drawn. The new star has now faded away to a mere speck of light, visible only in our largest telescopes. Unfortunately, and perhaps unavoidably, we have learned but little more from this new star concerning the nature of nebulæ and variable stars than we knew before its appearance. But the history of its discovery, its observation, and of its gradual fading out, is interesting enough to hold our attention for a little while.

The appearance of the new star was announced by Dr. Hartwig on August 31st, but it had been seen by him on the preceding night with a small telescope, and he had suspected some change in the nebula as early as the 20th of August, but bad weather, and a lack of instrumental means for making the matter certain, deferred the announcement until the 31st. From the various estimates at that time the star was probably a little brighter than the seventh magnitude, or just below the limit of visibility to the naked eye. The announcement of course turned a host of observers to the new star, and many erroneous estimates and statements were made. Some observers estimated the brightness far too great, and several, on account of errors in the observations, announced that the new star was moving with an enormous velocity. It required the lapse of a few weeks to clear away and correct all this error.

After Dr. Hartwig's announcement, it appeared that several others had seen the new star, but for some reason, perhaps want of familiarity with this nebula and lack of confidence that a new star had really appeared, they did not make a public announcement. Thus the Baroness Podmaniczky, of Eastern Hungary, saw the new star on August 22d or 23d, with a $3\frac{1}{2}$-inch comet seeker, and called the attention of a visitor to it, but they do not seem to have been certain that the object was new. This lady looked at the nebula on August 13, and did not see the new star. Mr. H. S. Moore, of McKinney, Texas, saw the new star on August 30. The circumstances indicate that this is a *bona fide* observation. A really independent discovery was made by Freiherr von Spiessen, of Winkel im Rheingau, who found the new star on August 30, and immediately sent a postal card to the Bonn Observatory where it was received on the evening of the 31st, or about a day before Hartwig's announcement. Mr. Isaac W. Ward, of Belfast, Ireland, claims that he saw the new star on August 19, when it was of the $9\frac{1}{2}$ magnitude. Finally, Professor Ludovic Grelly, of Rouen, says that he saw the new star on the 17th of August, and showed it to several friends and students. On the other hand Mr. Tempel, of Florence, Italy, who has done much work on nebulæ, and who is well acquainted with the great nebula

of Andromeda, says that he is confident there was no star in the place of the new one which was easily visible in his telescope on the 15th and 16th of August. This testimony is important, and serves to fix the time of the appearance of the *nova*, or at least the time it became an easy object in telescopes, within very narrow limits. This time must have been between the 16th and 20th of August, 1885. It is probable that the star increased rapidly in brightness, since on August 31st it was of the seventh magnitude. It never, I think, became much brighter, though statements were made early in September that it was of the second or third magnitude, and easily visible to the naked eye. Its diminution of brightness began about August 31, and has gone on pretty steadily until the present time. There have been several attempts to show that there were fluctuations in its brightness, but these are so uncertain that we must wait for the complete evidence.

At first, the position of the new star was confounded with that of the bright point of the nebula, and as this mistake added interest to the discovery, it was some time before it could be generally corrected. The coincidence with the nucleus of the nebula seemed to indicate that we had before our eyes the formation of a sun from nebulous matter, and hence a strong proof of the nebular hypothesis. Our popular astronomical writers were not slow to improve the occasion, but in fact the new star never coincided with the nucleus. The assumption of any intimate physical connection of the new star with the nebula has been given up by Vogel, of Potsdam, and Hasselburg, of Pulkowa, who have examined its spectrum. Within the limits of this nebula there can be counted from fifteen hundred to two thousand telescopic stars, and one of these has proved to belong to the class of temporary stars, so-called, of which we have records of from 20 to 30. What causes these stars suddenly to flame out, and then to fade gradually away we do not know; and so far as I know, there is hardly a plausible theory.

I first saw this new star on September 6, when its magnitude seemed to me 7½, and the star had a decidedly ruddy tinge. This color lasted but a few weeks, and as the star grew fainter it became of a white color. My observations have been continued until February 7th of the present year, and probably the star will be visible in the 26-inch refractor after the present moon has passed. It is now very near the limit of visibility in our telescope, or of nearly the sixteenth magnitude. The passage from the seventh magnitude to the sixteenth corresponds to a very great change of brightness, since it is the passage from the limit of visibility to the naked eye to that in a 26-inch telescope. Several hypotheses were proposed

to account for this wonderful star, and one that seemed to me quite ingenious is that of Mr. Monck, of Ireland, who assumed that this star is one of the swiftly moving ones that in rushing through the nebula had been set on fire, like a meteor in our atmosphere. Led by some such suggestions, and also by that of Professor Peters that it would be interesting to test the parallax of such a star, on September 29th I began some measures of the new star by referring it by means of polar coördinates to a known star of the eleventh magnitude, distant from it a little less than 2′. These measures have been continued until the present time, although of course as the star became extremely faint the measures became difficult and less accurate. I do not think my measures show any proof of a parallax, though they indicate perhaps a diminution of the distance, and even this may be sufficiently accounted for by variations in the light and color of the new star, since such variations would be likely to affect the measures. My measures are given in the following table, and I have added the parallactic coefficients computed from the formulæ,

$$\text{Coefficient for Angle} = [9{\cdot}7932].\ \cos(\theta - 172^\circ\ 21')$$
$$\text{Coefficient for Distance} = [9{\cdot}9810].\ \cos(\theta - 284^\circ\ 46')$$

where θ is the longitude of the sun; p denotes the observed angle of position, and s the observed distance.

Date.			p.	Coeff.	s.	Coeff.	Remarks.
			°		″		
1885,	Sept.	29	82·32	+0·602	109·98	−0·132	9th mag., sky hazy.
	Oct.	3	82·24	0·589	109·96	0·067	
		6	82·30	0·578	109·75	−0·017	Less than 9th mag.
		9	82·17	0·565	109·68	+0·032	10th mag.
		15	82·34	0·534	109·84	0·131	
		25	82·17	0·471	109·60	0·291	10·7 mag.
		26	82·25	0·464	109·57	0·307	
		31	82·36	0·426	109·80	0·384	
	Nov.	24	82·21	0·205	109·37	0·702	On Nov. 12th, mag. same as star of comparison.
	Dec.	2	82·21	0·122	109·27	0·783	
		6	82·28	0·079	109·31	0·818	11·5 mag.
		12	82·13	+0·014	109·27	0·862	
1886,	Jan.	1	82·25	−0·199	110·18	0·940	Very faint, misty; 15th mag.
		2	82·30	0·210	109·39	0 940	13th–14th mag.
		7	81·37	0·262	109 66	0·940	Very faint.
		30	82·27	0·460	108·67	0·846	Very faint, 16th mag.
	Feb.	7	82·17	−0·513	109·50	+0·779	Very faint, 16th mag.

The observations have been corrected for differential refraction, but the small reduction to a common epoch has been omitted. A comparison of the observed quantities with the coefficients shows little evidence of parallax. The observation of January 7 was made by Lieut. W. H. Allen, U. S. N., and

he estimated the brightness of the new star on November 12 to be the same as that of the star of comparison. Such a relative estimate is good, but the absolute magnitudes are uncertain. The limit of visibility in a 26-inch telescope is 16·3 magnitudes, and the *nova* is now so near this limit that accurate measures can no longer be made.

The great nebula of Andromeda is easily visible to the naked eye, and doubtless it was known to the astronomers of very ancient times. Those astronomers watched the heavens with unaided vision much more carefully than do modern astronomers, and they were far better acquainted with the constellations. The old astronomers had a theory that this nebula was variable both in form and brightness. They had poor means of judging of its form, but it is possible that their estimates of brightness may be more trustworthy, and that our new star may be an old variable which has appeared before, causing the nebula apparently to vary in brightness. One of the best descriptions of this nebula is by an astronomer of the middle ages, Simon Marius in 1612, who says it resembles a candle shining through a horn. It has a bright nucleus, around which is the soft, fleecy-looking nebulous matter, shading off very gradually from the center. This nebula has an angular extent of two degrees. No parallax has yet been found for a nebula; but if we suppose them to be as distant as the brightest of the fixed stars, and to have a parallax of a quarter of a second of arc, we may get some idea of the enormous extent of such a nebula as the great one in Andromeda. Making the preceding assumptions, its diameter will be about a thousand times as great as the distance of Neptune from the sun. Probably this estimate is less than the real diameter. That changes go on in such vast bodies we cannot doubt, for we see motion and change everywhere, but the distances are so great that small changes would pass without notice by astronomers on our earth. For this reason and also from the difficulty of making accurate drawings of such indefinite objects, we have as yet hardly any proof of changes in the forms of nebulæ.

U. S. Naval Observatory, 1886, Feb'y 12th.

ART. XXIX.—*On Some New Forms of the Dinocerata;* by W. B. SCOTT.

IN 1875, Professor Cope established the *Amblypoda* as an order of hoofed mammals, including as its two sub-orders the Coryphodons of the Wahsatch Eocene and the Dinocerata of the Bridger. This association of animals so divergent in appearance rests more especially upon the structure of the feet

and brain, and assumes that the two groups had a common ancestor, if indeed the one did not stand in an ancestral relation to the other.

If Professor Cope's hypothesis be correct we should naturally expect to find a series of forms connecting the two groups and leading to a common term, showing how the large and most curious Dinocerata could be closely related to the smaller and much less striking Coryphodons. In the latter there is a complete set of upper incisors and a canine tusk of moderate size; the lower incisors possess but a single lobe and the lower canine is erect and as large as the upper tusk, which it opposes. In the Dinocerata the upper incisors are entirely wanting, the canine is converted into a great sabre-like tusk; while the lower canine is very small, shaped like the incisors and functionally belonging to the latter, which present the extraordinary peculiarity of having compressed bilobed crowns. In the Coryphodons the cranium is nearly flat on top, there being no sagittal crest, and there are none of the great osseous protuberances which give such a characteristic and peculiar appearance to the skull of the Dinocerata, although in some species of Coryphodon (e. g. *elephantopus*) there are small swellings which indicate these protuberances; there is also a beginning of the supra-occipital and parietal crests which in Uintatherium reach such great proportions. The nasals are thin, short and weak, ending anteriorly in a point and strikingly different from the very long and heavy nasals of the Dinocerata.

The only form hitherto known which in any way helps to fill the gap between the two sub-orders of the Amblypoda is the genus *Bathyopsis* Cope, from the Wind river or lowest Bridger beds of Wyoming. Of this genus only the lower jaw has been found; but the important point is brought out that the lower canine was a large erect tooth, probably opposing the upper canine and not forming a part of the incisor series as is the case in Uintatherium. The form and position of this tooth make it exceedingly probable that the upper canine had not reached the great sabre-like proportions found in the other Dinocerata. It is not certain whether the presence of the first premolar is a constant feature or simply indicates a milk-molar persisting longer than usual. The latter is so frequently the case, that it is impossible to attach any value to its occurrence in an isolated specimen.

The Princeton Expedition of 1885 had the good fortune to discover, in the Bridger beds of Henry's Fork, Wyoming, another missing member of this hypothetical series. The new genus, for which I propose the name *Elachoceras*, may be briefly defined as follows: animals allied to Uintatherium, without upper incisors, and having six molars of the Uintatherium pattern, and large upper canine tusks; but without nasal pro-

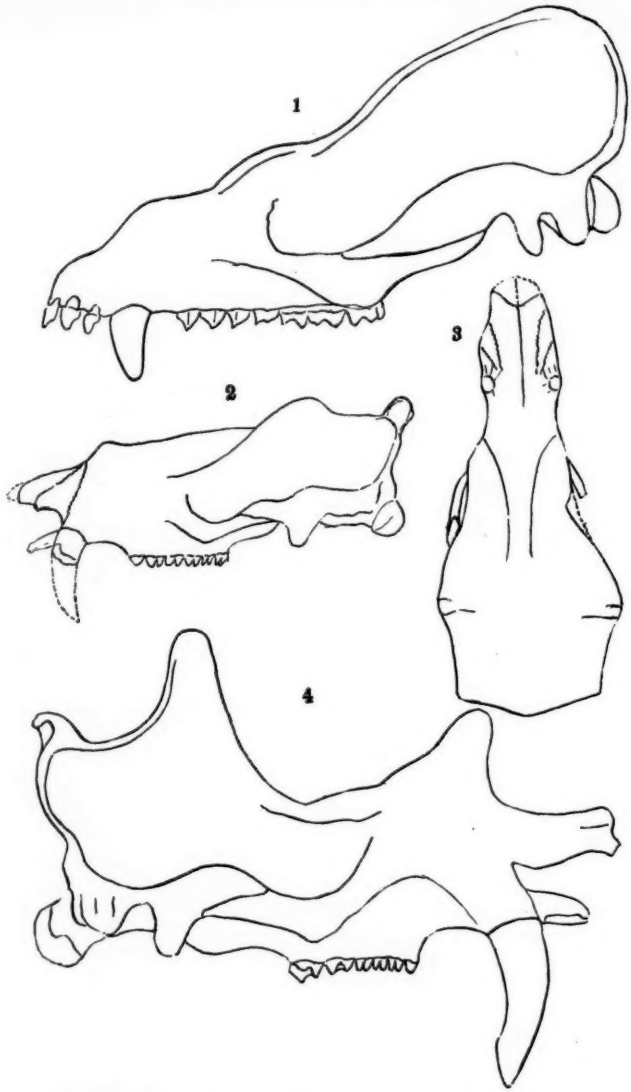


FIG. 1.—Skull of *Coryphodon*, left side (after Cope). FIG. 2.—Skull of *Elachoceras parvum*, left side. FIG. 3.—The same, top view. FIG. 4.—Skull of *Uintatherium alticeps*, right side.

tuberances and having only rudiments of the maxillary and parietal protuberances. The supra-occipital is pierced by two large venous foramina, placed one on each side of the median line.

The species may be called *E. parvum* and is defined by the low supra-occipital and parietal crests, the long and very narrow muzzle, the presence of a single tubercle on each molar tooth, and the small size of the animal (see figs. 2 and 3). It might at first sight be supposed that we have here the skull of a female or young animal, which would account for the very small size of the so-called horns. But the complete and somewhat worn dentition and the state of the sutures at once negative the supposition that the animal was not entirely adult. The question of sex is rather more difficult to decide. Professor Marsh has shown that the skulls of female Dinocerata are characterized by small canine tusks and less prominent "horns." Professor Marsh very kindly allowed me to make a careful examination of the female skulls in his collection, which immediately convinced me that Elachoceras is not a mere sexual variety of Uintatherium, as I had suspected might be the case. The tusks are not only proportionally but actually of much greater diameter than in very much larger females. In point of fact, in proportion to the size of the skull, the tusks of Elachoceras are nearly if not quite as large as in the largest Uintatherium males. On the other hand the protuberances are very much smaller than in any known female and the nasal pair seems to be altogether absent. I cannot, however, state this with entire certainty, as the extreme tip of the nasals is broken off, but the fact is more than probable for these reasons: (1) The nasals are preserved beyond the tips of the premaxillæ, where in all other known Dinocerata the swelling for the protuberance is visible. (2) In Elachoceras the nasals are exceedingly thin and weak, whereas the nasals of Uintatherium are strikingly strong and heavy; in the former there is no trace of any thickening or swelling at the tips of the bones.

Such a combination of large tusks with rudimentary protuberances is not what we find in any known female and seems to remove all reasonable doubt as to the sex of the specimen before us. If this be granted, the distinction of the genus from Uintatherium necessarily follows.

It is interesting to note that Elachoceras very much resembles the young specimens of the Dinocerata, especially the one described and figured by Professor Marsh (see his Monograph, p. 15, fig. 8), though even in this young skull the protuberances are much more prominent than in Elachoceras.

Another possibility is that in Elachoceras we have the missing skull of Bathyopsis; but this I consider to be extremely improbable, from the very peculiar character of the molar teeth

in the latter, which would almost certainly imply a similar modification of the upper molars. Another reason against such a reference comes from the presence of the large canine tusk of the upper jaw, which, as we have already seen, had probably not attained such dimensions in Bathyopsis. With the exception of the last named genus, Elachoceras is the smallest known member of the group, the skull measuring only about 22 inches in length.

In the same locality, though at a somewhat higher level, was found the large Uintatherium skull shown in fig. 4, which undoubtedly represents a new species of that genus, *U. alticeps*. Nothing is more hazardous than making new species of the Dinocerata, for, as every one who has studied them knows, they are extraordinarily variable, and if judged by the usual criteria almost every skull would constitute a distinct species. However, by carefully comparing nearly forty skulls, I have found that certain characters may be depended upon to determine the various species, not being subject to such apparently capricious variation. Among these may be mentioned the general shape of the skull, the *position* (not shape) of the "horns," the shape of the occiput, the character of the tusk, and the structure of the molar teeth.

Examined with reference to these characters the skull shown in fig. 4 is unquestionably distinct. It is one of the broad-headed species, with remarkably high occiput, the parietal protuberance is in *advance* of the post-glenoid process. (I have found the best method of exactly determining this point is to connect the tip of the pre-maxilla with that of the post-glenoid process and erect perpendiculars on this line.) The molar teeth are much as in *U. lucare*, but without tubercles on the anterior cingulum; there is a pair of small tubercles at the entrance of valley of the last molar. The lunar has no facet for the trapezoid. The great height of this skull, as compared with its length, suggested the name *alticeps*.

The value of the tubercles on the molar teeth as a specific character has been questioned, but I am inclined to think that considerable importance must be attached to them. For example, the very large series of Palæosyops remains in the Princeton Museum, ranging from species smaller than the sheep up to those as large as the rhinoceros, show without exception a small tubercle between the anterior cusps of the upper true molars. The tubercles on the teeth of the Dinocerata show no such constancy as this, it is true; nevertheless they are sufficiently constant to rank as species characters.

These notes are preliminary to a much fuller account which will shortly appear in a paper on the variations of the Dinocerata.

Geological Museum, Princeton, N. J., Feb. 12, 1886.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY.

1. *On the Volatility of Sulphur and Mercury.*—It is well-known that in the drying rooms of powder works where gunpowder is dried at a carefully regulated temperature which never surpasses 60° to 65°, there is always a peculiar odor somewhat recalling that of sulphur dioxide. By placing plates of glass in such a room at some distance from the mass of powder, BERTHELOT has succeeded in obtaining a distinct sublimate deposited on the glass, which gave, on analysis, sulphur 97·84, potassium nitrate 0·90, carbon and undetermined matters 1·26. Hence it is not an oxide but pure sulphur which has thus been sublimed at 60°, mixed, mechanically no doubt, with a minute quantity of the other constituents of the gunpowder. The vapor tension corresponding to this slow sublimation, effected 380° below the boiling point under normal pressure, is not appreciable by manometric measurements. It should be zero or sensibly so at the ordinary temperature, since no deposition of sublimed sulphur is observed in glass cases or tubes in which sulphur has been kept for many years. The permanent sharpness of the edges of sulphur-crystals in collections, also confirms its feeble volatility. The volatility of mercury, whose tension at 20° is only $0{\cdot}0268^{mm}$, the author illustrates by the fact that in his laboratory there is a mercurial trough and at a distance of 2·5 meters from it a glass case containing a bottle of iodine. After some years, Berthelot noticed that the neck of this bottle above its junction with the stopper was covered with red mercuric iodide. No ammonia nor volatile acid were kept in the case.—*Bull. Soc. Ch.*, II, xlv, 114, February, 1886. G. F. B.

2. *On Germanium, a new non-metallic Element.*—In the summer of 1885, Weisbach gave the name argyrodite to a new mineral existing in a rich silver ore from the Freiberg mines. On undertaking its analysis, WINKLER found it to consist of 73 to 75 per cent of silver, 17 to 18 per cent of sulphur, 0·21 per cent of mercury and traces of iron and arsenic. But in spite of all possible care, the loss in the analysis amounted to 6 or 7 per cent; and this without the detection by qualitative analysis of any other substance. Finally, the discrepancy was traced to the existence of a new element in the argyrodite, analogous to antimony, for which the author proposes the name Germanium. When the mineral is heated in a current of hydrogen, a black crystalline pretty easily volatile sublimate is obtained, which readily fuses into brownish-red drops, and which, besides a little mercuric sulphide, is essentially germanium sulphide. It dissolves readily in ammonium sulphide, and is precipitated by hydrogen chloride as a snow-white precipitate, readily soluble in ammonia. Heated in a current of air or treated with nitric acid, it gives a

white oxide, non-volatile at a red heat, soluble in alkali solutions, and reprecipitable by acids. Both oxide and sulphide are reduced by hydrogen and yield an element of a gray color and moderate luster resembling arsenic, volatile at a full red heat, somewhat more difficultly than antimony. Its vapor condenses in small crystals resembling iodine. Heated in a current of chlorine a white, easily volatile chloride is produced more volatile than $SbCl_5$ and whose aqueous solution is precipitated white by H_2S, after acidifying. The author has not yet determined the atomic weight, but suggests that the new element may fill the gap in the periodic system between antimony and bismuth.—*Ber. Berl. Chem. Ges.*, xix, 210, February, 1886. G. F. B.

II. Geology and Mineralogy.

1. *The Geology of Natural Gas in Pennsylvania and New York;* by C. A. Ashburner.—Mr. Ashburner observes that the oil and gas-yielding regions of Pennsylvania are geologically one. The borings yielding gas and oil go down to rocks in the Carboniferous and Devonian. About 30 miles west to northwest of Pittsburgh, at Smith's Ferry and Slippery Rock, the wells reach the base of the Coal-measures and Berea grit. To the north of Pittsburgh, 25 to 30 miles, they reach the Venango (Devonian) sands; and 30 miles S. 65° E. of Pittsburgh at Pleasant Unity, 31 S. 12° E, at Dunlap Creek, 45 m. to 48 m. south, at Whiteley Creek and Dunkard Creek, they go down to the Mahoning sandstone—the Lower Barren Coal-measures. The occurrence of accumulations of gas is stated to depend on the porosity of the rocks, the extent to which the rocks below the porous sand rock are cracked; the dip of the beds and the position of the anticlines and synclines; the relative proportion of water, oil and gas in the gas-bearing sand-rock; and the pressure under which the gas exists below.

The dip of the beds is very small, generally but 20 to 35 feet per mile, and that of 75 to 100 feet of the rarest occurrence. Of the three principal horizons yielding gas, (*a*) the Venango first oil-sand (or its probable representative) is 1800 to 1850 feet below the Pittsburgh coal bed, and believed to be in the Catskill formation; (*b*) the Sheffield gas-sand, the lowest in Warren Co., is of Chemung age; and (*c*) the Bradford oil-sand, which is 1757 feet below the base of the Pottsville conglomerate (or the lowest member of the Productive Coal-measures) is undoubtedly of Chemung age. But gas is obtained also from other horizons, up to that of the Mahoning sandstone, 500 feet above the Pittsburgh coal-bed. The gas is believed to have been formed in beds underlying the sand-rock from which it comes. The wells are commonly along the axes of synclines, but this position, Mr. Ashburner says is not universal.—*Amer. Inst. Mining Engineers*, Sept., 1885.

To the above facts from Mr. Ashburner's report, it may be now added that the very copious gas well recently opened at Findlay,

Ohio, is stated to descend into the Lower Silurian, and to afford gas at the rate of 40,000,000 cubic feet a year. The roaring of the escaping gas can be heard for five miles.

2. *Existing Glaciers in the United States;* by I. C. RUSSELL, U. S. Geol. Survey. From the 5th Annual Report of the Director, Major Powell. 1883–84. pp. 309 to 355. Washington, 1885.—Mr. Russell's Report, with its fine illustrations, is of interest to the country as well as to geological science. It gives the first connected account of the glaciers of the United States, and illustrates the subject with excellent views of some of the glaciers and glacier-bearing mountains from photographs. The glaciers have been called glacierets, because of their small size; but they have, still, as the views and descriptions show, the characteristics of those of greater extent. Mr. Russell's personal observations were made in the Sierra Nevada, on Mt. Dana, Mt. Lyell, and at other points in the High Sierras between the latitudes 36½° and 38°, at a height of about 11,500 feet above the sea. The division into the nevé and glacier is made out, and laminated or ribboned structure, dirt-bands and terminal moraines described. The dirt bands are stated to be probably a result, as suggested by Professor William H. Brewer, of the concentration, at levels, of surface dust by the periodical meltings over the glacier. The account of personal observations is followed by the earlier descriptions of the same glaciers by Muir and LeConte. Mr. Russell also gives in full Mr. Clarence King's description of the glaciers of Mount Shasta, made in 1870; and also an account by Mr. Gilbert Thomson, with fine views and a map of his observations, made on an ascent in 1883. To these are added Mr. Arnold Hague's account of the glaciers of Mt. Hood, in Oregon; Mr. S. F. Emmons's description of those of Mt. Rainier, in Washington Territory; notes on small glaciers of the Wind River Mountains, by Mr. W. H. Holmes; and on Alaska glaciers, by W. P. Blake and W. H. Bell. A general map of the Sierra Nevada region shows the positions of the existing glaciers, and also of the great glacier areas of former time.

3. *Upper Miocene* (*Loup Fork Beds*) *in Eastern Mexico.*—These beds, first described by Prof. de Castillo in 1883, occur on the borders of the States of Hidalgo and Vera Cruz. They have been found by Prof. Cope to afford remains of species of Protohippus, Hippotherium, and Mastodon, and probably of Procamelus; and others of Dicotyles are announced by de Castillo. The area is at least eighteen miles by six, and the thickness of the formation not less than 2,000 feet. It is intersected by the valleys of tributaries of the Tuxpan and Benados rivers, some of which are narrow gorges 1500 deep. Several thin beds of coal occur in it. This, Mr. Cope states, is the most southern point yet discovered of the Loup Fork beds.—*Amer. Nat.*, 1885, p. 494.

4. *New Carboniferous Arachnidan from Arkansas.*—Prof. S. H. SCUDDER refers to the Arachnidan genus *Anthracomartus* of Karsch, a fossil, somewhat trilobite-like in form, which he names

A. trilobitus. He observes that the *Brachypyge Carbonis* described by Woodward as the abdomen of one of the Brachyuran Crustacea is probably the abdomen of an Anthracomartus.

5. *Revision of the Palæocrinoidea* by C. WACHSMUTH and F. SPRINGER.—Part III of this important memoir published in the Proceedings of the Academy of Natural Sciences, has been completed. It covers 138 pp. 8vo, and is illustrated by plates IV to IX inclusive.

6. *Geology — Chemical, Physical and Stratigraphical;* by JOSEPH PRESTWICH, M.A., F.R.S., F.G.S., Prof. Geol. Univ. Oxford. In two volumes: Vol. I, *Chemical and Physical Geology.* 477 pp. 8vo.—Professor Prestwich's long labors in geology have supplied him with much material of his own observation for his geological treatise, and have enabled him to prepare a work that will contribute largely to the progress of the science. The volume now issued, after an introductory chapter, takes up the subject of the constituents of the earth's crust—its rocks. A review of the animal and vegetable kingdoms follows. Afterward the work treats of the formation of sedimentary deposits and the atmospheric and other agencies concerned, of ice-action, volcanoes, earthquakes, and coral islands. Then follow chapters on the disturbances of rocks with their effects, on mountain-ranges, on metalliferous deposits and veins, on igneous rocks and metamorphism. The various principles of the science are explained through well-selected facts and illustrated by numerous excellent figures, many of which are new to text-books. Among the latter, the one of the interior of Kilauea, we should like to modify. Besides figures in the text, there are also folded plates of sections, and three folded maps of the world, which add much to the value of the work. Of the latter, one is a colored map of the geological formations; another, colored, shows the oceanic currents and the distribution of coral reefs and islands; the third gives the distribution of volcanoes. The volume is from the Clarendon Press, Oxford.

7. *Artificial minerals.*—M. A. DE SCHULTEN, who has already accomplished many interesting results in chemical mineralogy, has recently succeeded in forming artificially the hydrous iron phosphate compound, $Fe_2P_2O_8+4aq$, which exists in nature as the mineral strengite; the rose-colored crystals obtained had a specific gravity of 2·74 and unlike strengite seemed to be monoclinic in crystallization. The method employed was to heat in a sealed tube, at 180° to 190° C., 26 cc. of a solution of the salt Fe_2Cl_6+12aq with 4 or 5 cc. of a solution of a specific gravity 1·578.

The same chemist has formed a hydrate of magnesium in small distinct hexagonal crystals (the mineral brucite), and also made a crystallized hydrate of cadmium, in flattened hexagonal prisms, optically uniaxial.

8. *Palæontology of New York;* by JAMES HALL. Vol. V, Part II, Lamellibranchiata.—The Plates of Vol. V, Part I of

Professor Hall's volume of plates of the Palæontology of New York, illustrating the Lamellibranchiata of the Upper Helderberg, Hamilton and Chemung groups, containing, with a few omissions, 80 plates, was issued in 1883, and is noticed in this Journal for that year (vol. xxv). Since then, Plates 81 to 92 inclusive have appeared, and now we have Plates 35 and 42, and 93 to 96 inclusive. The text of the second part of Vol. V, Part I, has also been issued, making the whole complete. Professor Hall states that the number of species of American Paleozoic lamellibranchs now known is over 1250; and that nearly half of this number have been collected in New York from the Trenton and later Paleozoic strata, and 500 from those above the Oriskany. The study of the species was a work of great labor and difficulty, and Mr. Hall is to be congratulated that the printing is finished. All paleontologists will rejoice with him, and give due honor to him and to the State of New York for this very valuable contribution to American science.

The Annual Report for 1882, of "the State Geologist," Mr. Hall, (in 4to, instead of the usual 8vo form), is made up chiefly, after a paper on the mode of growth of Fenestellæ, of plates of Fossil Corals and Bryozoans of the Lower Helderberg and Bryozoans of Upper Helderberg, 33 in number, and 28 plates illustrating the genera of some families of Brachiopods, being part of a revision of the Paleozoic genera of this group—portions of volumes of great paleontological interest yet to be published.

III. Botany and Zoology.

1. *Botanical Necrology for* 1885.—Supplemental to the bibliographical notices given in the January number of this Journal, pp. 13–22, we should record the following obituaries:

Jean-Etienne Duby, long one of the Genevese clergy and not undistinguished as a botanist, died at Geneva, Switzerland, November 24, 1885, at the age of 88. In the year 1828 he edited the second edition of DeCandolle's Botanicon Gallicum, which served an important purpose as the manual of French and Swiss botany for many years. In 1844 he produced his memoir on the *Primulaceæ*, and his elaboration of that family for the Prodromus. His other publications relate to the lower Cryptogamia. They began in 1829 with a paper on the application of the principles of taxonomy to a tribe of Algæ: the latest, that we know of, was upon some new or little understood *Musci*, and was published in 1869. With this venerable man passed away the last of the colleagues of Aug. Pyramus de Candolle.

The Brothers Tulasne. The elder brother, a botanist of remarkable acuteness and sagacity, Louis René Tulasne, was born on the 12th of September, 1815, and died at Hyères, on the 22d of December, 1885, twenty years after he had abandoned all scientific pursuits. Charles, born a year later, who published no independent work, but drew most of the excellent figures

which illustrate those of his brother, and was associate-author of some of them, died a year and a half earlier. Both were devoted Roman Catholics, and it is understood that, in giving up botany on account of broken health, they retired to a specially religious and recluse life, filled with acts of charity.

Between the years 1841 and 1865 L. R. Tulasne was a prolific author, and his work was always excellent in matter and in finish. Excepting a single essay upon embryology, the phænogamic papers and memoirs all relate to systematic botany. The most considerable of them are his classical monographs of *Podostomaceæ* and of *Monimiaceæ*. The mycologists tell us that his Fungi Hypogæi and the Selecta Fungorum Carpologia are the most important works of the age in that department. A. G.

2. *Drugs and Medicines of North America, a publication devoted to the Historical and Scientific Discussion of the Botany, Pharmacy, Chemistry and Therapeutics of the Medicinal Plants of North America, their Constituents, Products, and Sophistications.* Vol. I. Ranunculaceæ. J. U. Lloyd, Commercial History and Pharmacy. C. G. Lloyd, Botany and Botanical History. Cincinnati, 1884–85, pp. 304, imp. 8vo.—With the ninth fascicle this volume is completed, and an index, new title-page, etc., are given. It appears that, besides the very numerous figures (over one hundred), which are scattered through the letter-press, there are twenty-five full-page illustrations, chiefly figures of the plants under consideration, and very good ones too. An idea of the thoroughness and extent of the work may be had by considering that this whole volume is devoted to the plants of one order, the *Ranunculaceæ;* and "it cannot be denied that a plan so comprehensive has involved great expenses and difficulties." One reads, therefore, with a gratification not unmixed with wonder, the courageous announcement that: "We next take up the succeeding natural orders, and hope to continue until we have completed the subject."

Having already given a notice of the earlier parts of this volume, now brought to completion, we have only to state that the last two fascicles are devoted to *Cimicifuga* and to *Xanthorrhiza*. The authors will notice that we follow their orthography of the name. It is the unquestionably correct form. The only reason why it has not been altogether adopted is that the form given by L'Heritier, *Zanthorhiza*, was supposed to have been earliest in publication, and botanists are not allowed much discretion in mending faulty names. But as the origination of the name is generally attributed to Humphrey Marshall, and as he published it with the correct initial, in the same year (1785) which is borne on the title-page of that fascicle of the *Stirpes Novæ* by L'Heritier, who prints it with a wrong initial letter, no rule of priority is violated in the correction of a faulty orthography. This may be done without charging, as the authors of this volume do (in a foot-note), "that L'Heritier deliberately stole the name and antedated its publication." They have themselves antedated it, by

giving 1784 as the date of L'Heritier's publication: that fascicle of the *Stirpes Novæ* bears the date of 1785. He got the plant from the English gardens, to which it came from Bartram, and took up the name under which, doubtless, it came to him. Between a wrong and a right form of the same name, under the same date, there is no question which should be employed.

The genus being anomalous in *Ranunculaceæ*, and having a woody stem, containing plenty of berberin, our authors opine that the genus should be transferred to the Barberry family. But the floral characters would be far more incongruous there; and it would be carried away from *Hydrastis*, which has the same yellow coloring matter and also contains berberin. A. G.

3. *Leerboek der Planten-physiologie*, door HUGO DE VRIES. Amsterdam: Brinkman, 1880. pp. 300, 8vo.—This is the physiological and histological part of the *Leerboek der Plantenkunde*, i. e., the Botanical Text Book of Oudemans and the present author, with which the Netherland students are highly favored. It is out of print; but when the expected new edition appears, it would be well if some one would translate it into English. For Professor de Vries is as excellent in exposition as in research. We turn to this volume just now because the author is one of the most notable investigators of plant-movements, and was among the first to indicate the correlation of these movements with growth. If we rightly remember, he was at first inclined to regard the movements as a phenomenon of growth. But he was too good an observer to rest in that opinion, the total improbability of which must be manifest upon a survey of the whole field. For obvious reasons, visible movements would in general be manifested only in growing or freshly grown organs, and varying turgor of the protoplasm of the cells concerned, which causes the visible movements, must needs go along with growth. But when the tendril of a *Sicyos* incurves or coils promptly upon a light touch, straightens after a brief interval, and coils again upon renewed touch, and when as a result of repeated irritation, the organ becomes weak and flabby instead of rigid, one would say this is no more the result of alternate growths on the two sides than are the movements to and fro of the leaflets of *Desmodium gyrans*. As De Vries states it, while growth and turgor may be intimately associated, in very many cases there is no permanent elongation of the part, but only a temporary lengthening due simply to increase in turgor; that in others, when accompanied by growth, the latter is insufficient to account for the wide movements; and that in still other cases, where there is no recognizable growth, the movements are attributable to variations in turgor alone. He suggests, in continuation, that "the elongation which cell-walls undergo in these movements through turgor generally, may exert an influence upon intussusception by which new solid molecules are thrust in between previous existing ones in the direction in which elongation has taken place; consequently the elongation, at first caused by turgor only, may after-

ward become independent of it. The extended observations of Professor Penhallow, recorded in the three preceding numbers of this Journal, may be interpreted in accordance with these views. We should say that they do not necessitate the quite different conclusions he draws in his summary on page 189. A. G.

4. *Baillon's Dictionnaire de Botanique* we now have down to the nineteenth fascicle, and down to the latter part of the letter G, with the accustomed fullness of articles of every sort, germane to the subject, and with the usual wealth of illustrations. See for example the article *Fruit.* A. G.

5. *Baillon's Histoire des Plantes.*—After considerable delay we now have the closing part of the eighth volume, but with title-page to the parts only, none of the volume itself; and the same is to be said of the two last preceding volumes. The present part, which goes with the *Compositæ*, is a monograph of the *Campanulaceæ* (in a very extended sense), *Cucurbitaceæ*, *Loasaceæ*, *Passifloraceæ*, and *Begoniaceæ*. A. G.

6. *Plants naturalized in the southwest of France: Recherches sur les Plantes naturalisèes dans le Sud-Ouest de la France;* par Joseph Lamic. An exhaustive article of 122 pages in the Annales des Sciences Naturalles de Bordeaux et du Sud-Ouest, tom. iv, 1885. —The basin of the Garonne, and especially the neighborhood of Bordeaux and Bayonne, is thought to have received a larger infusion of foreign plants, in proportion to its indigenous species than any other part of France, or even of Europe. Dr. Guillaud estimates them at about one-thirtieth of the Phænogams of the flora. M. Lamic counts them at 80 out of 2500. In this essay, after sketching the features of the region (ancient Aquitania), and defining what he means by a naturalized plant (not hesitating to include those which multiply by budding, if they are fully established), and considering the degrees, proofs, and means of naturalization, he takes up these denizens in order, and tells us what is known of the mode and time of introduction. We will note some of the North American species.

Lepidium Virginicum, L.—This was detected at Bayonne within the half century, and up to 1868 was unknown elsewhere in France. It has spread widely since the introduction of railways, and doubtless by their aid, partly through increase and continuity of its favorite habitat, *in glareosis.*

Senebiera pinnatifida, DC., or *didyma*, Pers.—This is attributed to the Southern United States, but its home is probably farther south.

Hibiscus Moscheutos, L.—The history of the identification of *H. roseus* of S. Europe with this species is given in full detail. There is no documentary evidence of its introduction into Europe where it has been known (in Italy) for 350 years. If our author were acquainted with it in its native country he would have dismissed his lingering doubts as to its North America origin.

Rhus typhina, L.—Propagates by seed and is establishing itself in southwestern France.

Robinia Pseudo-acacia, L.—Alphonse de Candolle would not admit this to be really naturalized in Europe, on the ground that it multiplied only by the root. Mr. Lamic asserts that to his knowledge, it spontaneously propagates by seeds as well.

Gaura Lindheimeri, Engelmann and Gray (by some oversight attributed to Linnæus) finds a place in this list because it was found growing wild near a railway station in the Landes. Dr. Guillaud thinks it came with American grain which had been unloaded there. This seems to us very improbable. It is more supposable that it was an accidental escape from cultivation.

Solidago Canadensis, L., *S. glabra*, Desf., *Erigeron Canadense*, *Aster Novi-Belgii*, &c., need not be remarked upon; *Boltonia glastifolia* is a recent arrival, but at some places near Bordeaux it seems to have come to stay.

Vittadinia triloba, DC.—M. Lamic says this is a native of New Zealand; he should mean New Holland. But his plant doubtless is *Erigeron mucronatum*, of Mexico, which persists in European gardens under this totally wrong name, long since corrected.

Xanthium macrocarpon, DC., which we suppose is one of the forms of *X. Canadense*, Mill., though perhaps not original to so northern a region, was known in Europe only around Montpellier in 1814; it is now largely diffused through a wide region.

Asclepias Cornuti, has recently naturalized itself in Aquitaine, where it spreads freely by its deep root-stocks. Lamic refers its home to the southern Atlantic States; we should refer it rather to the northern.

Sagittaria obtusa, Willd.—The male sex of this species has in some way been transported from N. America to the waters of the Bordeaux district, where it is spreading widely, although it never fructifies; in this respect it follows the example of *Anacharis Canadensis*, which has also occupied the waters of the whole district.

Juncus tenuis, Willd., is also a late naturalization. Lamic reckons Georgia and Carolina as its home: but its range is more largely northern.

From their omission, it would appear that *Elatine Americana* and *Ilysanthes gratioloides*, which have somehow found their way to the region of the Loire, have not reached the basin of the Garonne. A. G.

7. Professor Edward Tuckerman, one of our oldest and best botanists, and probably the most profound and trustworthy lichenologist of the day, died at Amherst, Massachusetts, on the fifteenth of March. A. G.

8. *Adaptation to an emergency in the Cicada septendecim.*—Dr. J. S. Newberry, in the School of Mines Quarterly (vol. vii, January, 1886) describes a singular case of adaptation to a new condition in the Seventeen-year locust. A surface of ground, in the outskirts of Rahway, N. J., in which the larvæ of the locust had buried themselves, as the sequel shows, was afterward built over. The cellar of one of the houses, which was without a floor

over the earth at bottom, had been kept closed and dark until about the time for the escape of the Cicadas. When opened, it was found thickly set with tubular mud-cones, 6 to 8 inches high and 1 to 1½ thick. The bottom had been dug over to a depth of a foot into red clay, when the house was made, so that it is certain there was nothing there of the kind then. At the time of the opening, the tops of the cones were closed, and on breaking them the pupæ were found inside. After the Cicadas had appeared, holes were found in the tops.

Dr. Newberry received specimens of the cones from four citizens of Rahway, who testified to the facts as to the cellar. The club-shaped cones are roughly made of pellets of the clay. Dr. Newberry observes that the Cicadas, finding the place dark, and apparently desiring to work up to daylight, made the pellets of moist clay and with them built up the tubes, as if "for the purpose of bridging over the vacancy, and thus reaching the surface." What schooling or experience could have "fitted the Cicadas for the engineering work they attempted" Dr. Newberry leaves for others to explain. An excellent figure of the club-shaped cones accompanies the paper.

9. *Bulletin of the Scientific Laboratories of Denison University.* Edited by C. L. HERRICK, Prof. Geol. and Nat. Hist. 136 pp. 8vo, with several plates, and an Appendix of Tables for the determination of minerals. Granville, Ohio.—This first Bulletin from Denison University contains the following papers, showing remarkable scientific activity at the institution: On the Evening Grosbeak, *Hesperiphona vespertina*, with a colored plate of the bird, and a plate illustrating details in the osteology, by Professor Herrick; Metamorphosis of Phyllopod Crustacea, with five well-drawn plates, by the same; Superposed buds, with one plate, by A. F. Foerste, of the university; Rotifers of America, Part I, with a description of a new genus and several new species, and four plates, by Professor Herrick; the Clinton Group of Ohio, with descriptions of new species of fossils, and two plates containing figures of nearly forty species of fossils, by A. F. Foerste. The lithographic plates of the volume were all made by Professor Herrick. The expenses of the volume were met by the contributions of friends of the university. The material is ready for another number, which they hope to meet from the proceeds of the present volume, for which the charge is $1.25.

10. *Evolution versus Involution:* a Popular Exposition of the Doctrine of true Evolution, a refutation of the Theories of Herbert Spencer and a vindication of Theism; by A. Z. REED. New York, 1885. (James Pott & Co.).—The author of this work means well; but his subject is evidently too deep and broad for him.

IV. ASTRONOMY.

1. *Uranometria Nova Oxoniensis, a photometric determination of the magnitudes of all stars visible to the naked eye from the pole to ten degrees south of the equator;* by Professor C.

PRITCHARD. 8vo. Clarendon Press. Oxford, 1885.—This volume contains the results of Professor Pritchard's measurements during several years past of the light of the stars by the use of the Wedge Photometer. This instrument is a wedge of very nearly neutral tinted glass six and a half inches long, an inch broad, and 0·145 inch thick at one end, and tapering to ·02 inch at the other. The two places in the wedge at which the light of two stars is extinguished having been observed, the distance between them is taken as a direct measure of the difference of the magnitudes of the two stars. The stars selected are those of Argelander's Uranometria Nova.

The Harvard Photometry was published when the measures for the Uranometria were about three-fourths completed, so that Professor Pritchard has been able to compare his results with Professor Pickering's in the notes.

The Royal Astronomical Society, of London, has awarded Copley medals to both Professor Pickering and to Professor Pritchard.

2. *Nebulæ in the Pleiades.*—On the 16th of November Messrs. Paul and Prosper Henry of Paris discovered a nebula close to Maia upon their photograph of the Pleiades. It had a well-marked spiral form and seemed to proceed from the star. It was about 3′ in extent. It could not be seen in their telescopes.

The nebula was, however, afterward seen by M. Struve in the newly mounted 32-inch refractor at Pulkowa. Markings of the same nebula have since been found by Professor Pickering upon a photograph taken by him at the Harvard College Observatory, Nov. 3d. These had been assumed by Professor Pickering to be due to defects in the photographic process, but the irregularities of light correspond so closely to what is described by Messieurs Henry that there can be no doubt as to their origin. The photograph also shows markings corresponding to the disputed nebula about Merope. A faint narrow streak proceeds from Electra also. No nebulous light is noticeable about Alcyone, Atlas, Pleione, or Taygeta.

3. *Relation of the Zodiacal Light to Jupiter.*—Dr. H. GEELMUYDEN, of Christiania, in a letter speaks of Professor Searle's researches upon the zodiacal light (see this vol., p. 159). He says: "If the zodiacal matter has the same position among meteoric matter in general as comets of short period among comets, it is to be expected that the fundamental plane of the zodiacal light will have some relation to Jupiter as the principal motor in deflecting the orbits, and therefore in collecting the matter. Now it is worth remarking that the most northerly point of Jupiter's orbit has the heliocentric longitude 188°, or with 60° east elongation 178° geocentric longitude; and for matter in the same plane, but nearer the Sun, the approximation to coincidence with 160° is still greater."

4. *Relation of Asteroid Orbits to those of Jupiter.*—In Mr. Searle's paper (see this vol., p. 160) it is shown that the asteroid orbits have some relation to the plane of the zodiacal light. These

orbits should have a relation to the orbit of Jupiter. For, suppose the orbits of the asteroids to be distributed in any manner whatever, provided only that they shall make small angles with the plane of Jupiter's orbit. The action of Jupiter should give to each orbit a motion of its node, and these motions of the nodes will not be the same for the different orbits. After the lapse of a considerable time the orbits will thus evidently come to be distributed somewhat symmetrically about Jupiter's orbit.

This relation is moreover easily shown to be true in fact. For if we take on the celestial sphere the poles of the 251 known asteroid orbits and compute the center of gravity of these as points of equal weight, that center of gravity is found to be only 30′·0 from the pole of Jupiter's orbit.

Regarding this center of gravity as the pole of the *mean-plane* of the asteroid orbits, we may say: that *the plane of Jupiter's orbit lies nearer to the mean-plane than does any single asteroid orbit-plane to the mean-plane.* The asteroids whose orbits have inclinations to the mean-plane less than one degree are Medusa and Euterpe, whose inclinations are 46′ and 49′.

Only one asteroid orbit-plane lies nearer to Jupiter's plane than does the mean-plane to Jupiter's plane. The asteroids whose orbits are inclined less than one degree to Jupiter's plane are Euterpe, Elsa, and Vanadis, whose inclinations are 19′, 43′, and 55′. H. A. N.

5. *Photographs of the Solar Spectrum.*—The announcement has been recently made that the photographic map of the normal Solar Spectrum, made by Professor H. A. Rowland with one of his concave gratings, is nearly complete. Seven plates on heavy albumen paper are offered for sale; each contains two strips of the Spectrum, and one of them gives three; they are three feet long and one foot wide. The region covered by these plates extends from wave length 3100 to 5790. Those interested are already aware of the unique value of these photographs. The price for the set is $10, or $12 if mounted on cloth, and for a single plate $2 and $2.25 respectively. Orders should be sent to the Publication Agency of the Johns Hopkins University, Baltimore, Md.

V. Miscellaneous Scientific Intelligence.

1. *Report of the International Polar Expedition to Point Barrow, Alaska.* 695 pp. 8vo. Washington, 1885.—This volume contains an account of the results obtained by the United States expedition to Point Barrow in 1881, 1882, and 1883. The narrative of Lieutenant Ray is brief but interesting, and is followed by a chapter by him giving an ethnographic sketch of the natives; both of these are accompanied by a number of excellent full-paged phototypes, of the northern scenery, ice phenomena and also portraits of the Eskimos. Part IV is devoted to the mammals, birds, etc. This is written by John Murdoch except the report on Mollusks, by W. H. Dall. The remainder of the volume is devoted to the subject which formed the special study of the

expedition, viz: the meteorological, magnetic and tidal observations; this record is very full and may be expected to yield important results, especially when digested in connection with the observations made by other of the International Polar Expeditions at their respective stations.

2. *Third Annual Report of the Bureau of Ethnology to the Secretary of the Smithsonian Institution*, 1881–'82, by J. W. Powell, Director. pp. lxxiv and 606, 4to, with numerous colored and uncolored plates and other illustrations.—The third annual report of Major Powell is, like its predecessors, a valuable contribution to Ethnology. The volume contains, besides the Director's report, which forms the introduction, papers on the following subjects: On certain Maya and Mexican manuscripts, by Cyrus Thomas; on masks, labrets and certain aboriginal customs, by W. H. Dall; on Omaha Sociology, by J. Owen Dorsey; on Navajo weavers, by Dr. W. Matthews; on prehistoric textile fabrics of the United States, derived from impressions on pottery, by W. H. Holmes; also two illustrated catalogues of collections made in 1881, by W. H. Holmes, and by James Stevenson.

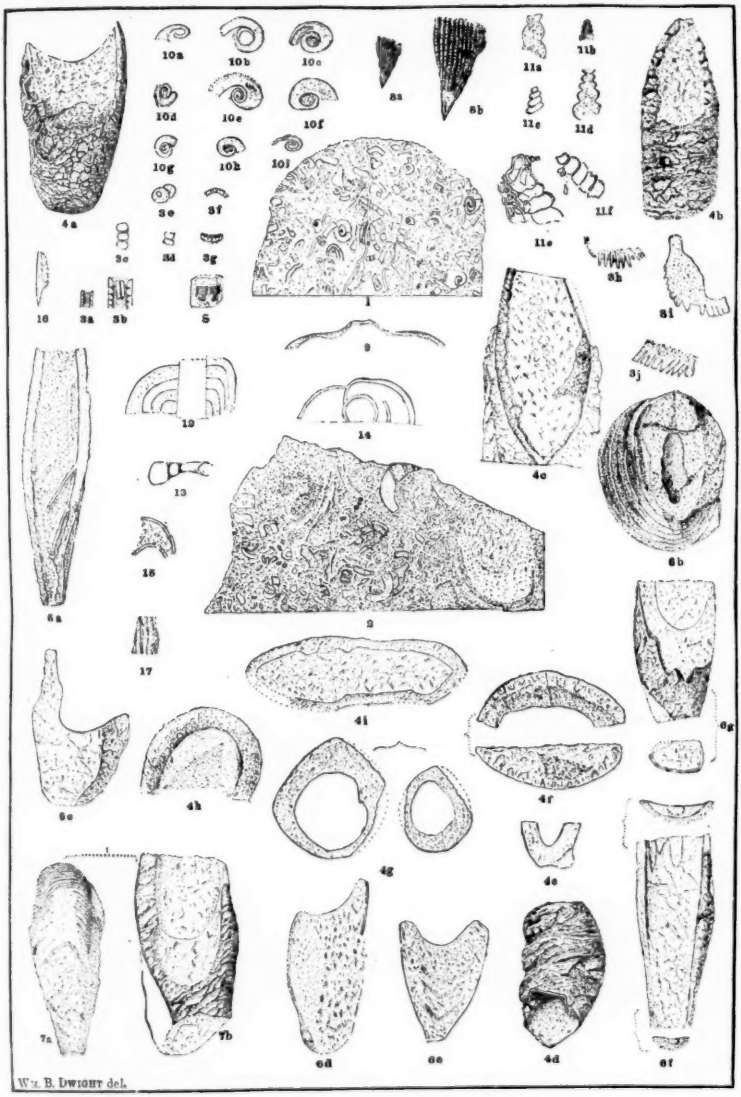
3. *Thermometer Exposure;* by Henry A. Hazen. 32 pp. 4to, Washington, 1885, (Professional Papers of the Signal Service, No. xviii.)—This memoir contains an extended discussion of the subject treated by the same author in an article in this Journal in 1884 (vol. xxvii, 365); it concludes with an illustrated description of the roof thermometer shelter adopted by the U. S. Signal Service.

4. *Wöhler Memorial.*—An urgent appeal is being made anew in behalf of the fund for the statue to be erected at Göttingen in commemoration of the life and work of the eminent chemist Friedrich Wöhler. This fund now amounts to about $4,000, but must be considerably increased before it will be sufficient for the end in view. The circular of the committee asks for aid, by individual subscription and influence, from all in this country interested, especially from the chemists. Prof. J. W. Mallet of the University of Virginia is Chairman, and Prof. Ira Remsen of Baltimore, Secretary and Treasurer.

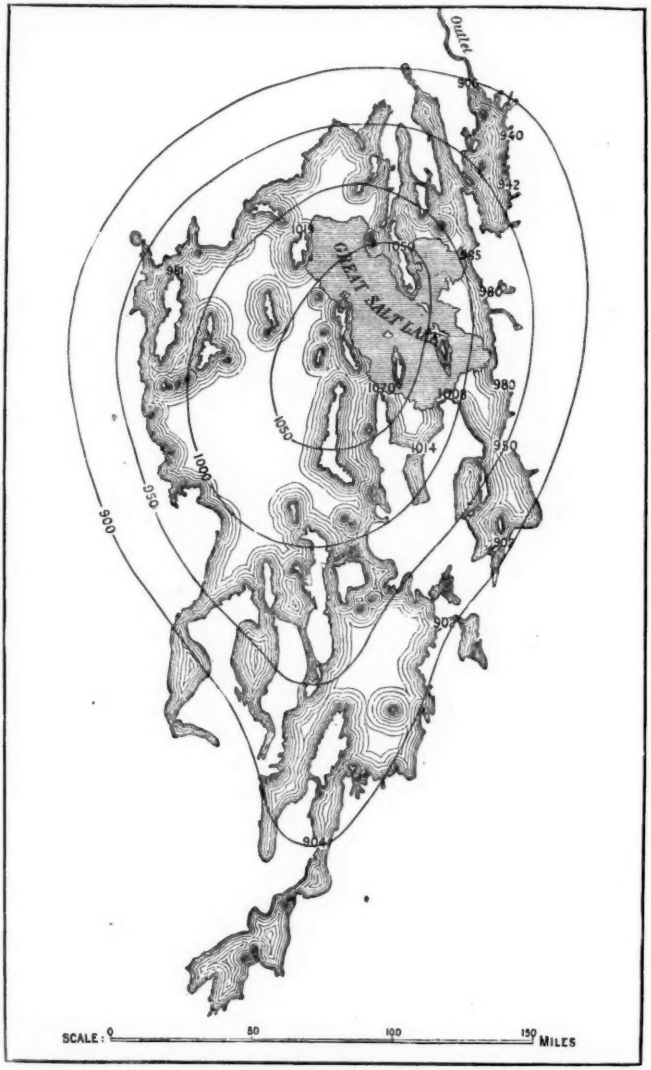
OBITUARY.

A. von Lasaulx, Professor of Mineralogy at the University of Bonn, died on the 25th of January, at the age of forty-six. He was one of the most active workers in Germany in the departments of Mineralogy and Petrography, and his early death is a severe loss to science.

Heinrich Fischer, Professor of Mineralogy at the University of Freiberg in Baden, died in February last. His most important contributions were in the line of microscopical mineralogy, and to the study of the various minerals included under the name jade he devoted himself most earnestly, both from the mineralogical and archæological side. His most important work was a volume of 400 pages upon "Nephrite and Jadeite" published in 1875, and a second edition in 1880.



FOSSILS FROM CANAAN, COLUMBIA CO., N. Y.



Map of the extinct Lake Bonneville, with hypothetic contours to illustrate the subsequent deformation of the earth's crust, by G. K. Gilbert. The horizontal figures mark determinations of the height of the old shore-line above Great Salt Lake.